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CHEMISTRY,

AND

THE ARTS.

VOL. XI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Volume, are Mr. Thomas Reid; Mr. James Scott; Mr. Boswell; Mr. John Gough; Mr. Irvine; Mr. Cuthbertson; Mr. William Henry; John Bostock, M. D.; W. Brande, Esq.; Mr. Matthew Murray; Mr. J. C. Hornblower; Mr. A. F. Thoelden; Mr. John Clennell; J. P.; W. N.; W. F. S.; Mr. William Wilson; A. Thomson, Esq.; Amicus; Mr. O. Gregory; Mr. Ezekiel Walker; Mr. James Stodart; Count Rumford, V. P R. S.; Mr. Thomas Harrison. A Constant Reader.

Of Foreign Works, Mr. Erman; M. Bonnard; Messrs. Robertson and Sacharoff; Lalande; A. M.; Conſtat Duméril; C. L.; A. B. Berthollet; J. C. Delametherie; M. Bralle; Profeſſor Pini; Mr. Deyeux; M. Hassenfratz; Mr. Goettling; Mr. Steinacher; Mr. Marechaux; Mr. Schnaubert; Mr. J. G. Schmidt; Brugnatelli; Citizen Duhamel; C. L. Cadet; J. Machlachlan; Humbold and Bonpland.

And of English Memoirs abridged or extracted; Charles Hatchett, Esq. F. R. S.; Mr. C. Waistell; Mr. George Dodd; Dr. William Roxburgh; Captain Joseph Brodie; Mr. William Hardy; Mr. James Rawlinson; Humphry Davy, Esq. F. R. S.; Richard Chenevix, Esq. F. R. S. M. R. I. A.; Mr. Robert Seppings; A. Carlisle, Esq. F. R. S.; John Churchman, Esq. Imp. Acad. of Sciences, Petersburgh; Mr. D. Mushett, Sir A. N. Edelcrantz; Edward Bigott, Esq.

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Soho Square, London, September 1, 1805.

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A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

MAY, 1805.

ARTICLE I.

Letter from Mr. THOMAS REID, *on the Conſtruction of Time-keeping Machines.*

To Mr. NICHOLSON.

SIR,

IN your intereſting and uſeful Journal of December laſt, I was glad to ſee the improvement of compenſation pendulums for aſtronomical clocks, ſo zealouſly taken up by ſuch an able hand, as that of Mr. Edward Troughton's.

Compenſation pendulum of Mr. Edward Troughton.

But an excellent clock of this ſort becomes ſo very valuable and neceſſary an appendage in an obſervatory, to thoſe aſtronomical inſtruments with which he is ſo happily engaged in daily conſtructing and improving; that he muſt more readily ſee their advantage than even thoſe whoſe buſineſs it is to make ſuch clocks. If Mr. Berthoud, a celebrated author on every part that regards the improvement of time-keeping machines, is correct, it would appear, that the ſteel wires of Mr. Troughton's pendulum muſt be too ſlender. Mr. Berthoud, by his experiments, ſaw, that there was a certain ſtrength of materials neceſſary, in order to render the compenſation complete having found, that on the pendulum rods (if too ſmall) being lengthened by heat, the contraction by cold would not

Probability from Berthoud's experiments that the ſteel wires may be too ſlight.

ball again quite up to the place where it ſet out from e heat was firſt applied, and this with a ball of a weight; no doubt the weight of the ball may be ſervient to any ſize of wires.

Mr. Troughton ſeems t[illegible] attended this.

ier Mr. Troughton has attended to theſe circumſtances, not, but ſuſpect from his propoſing, yet, to make ſort of pyrometrical apparatus for the further proving of his pendulums, that the complete compenſation has not been fully aſcertained, only in ſo far as regards the calculation of the relative expanſions of braſs and ſteel.

The improvement of clocks, and time-keeping machines of every deſcription, more particularly thoſe deſtined for aſtronomical purpoſes, is a ſubject that has not a little engaged my time and attention.

The pendulum of Ludlam with a wooden rod examined.

There is a pendulum, having a wooden rod, the conſtruction of which is deſcribed with great neatneſs, perſpicuity, and mechanical knowledge, by its author, (Mr. Ludlam, late an eminent profeſſor at Cambridge) in his eſſays, and recommended by him, *who was no mean judge*, as one of the beſt in almoſt every reſpect, particularly in ſo far as regards the impulſe from the clock taking place through the middle line or centre of the rod, to be thence communicated in the ſame line to that of the ball, that hence no circumgyratory motion ſhould take place. Now, although the principle ſet out with *here*, ſeems completely adapted to prevent this ſort of motion, it will be found on trial, as I did, that of all pendulums yet made, it is the moſt liable to generate this very ſort of motion.

It is liable to a ſide oſcillation of the ball round the rod as an axis.

The ball being the middle fruſtum of a globe, a form whoſe matter is much ſpread out from the centre to the edge, and having a large hole, for the rod to paſs through; *this* taking away much of the matter from the centre, tends much more than the lenticular form, to produce the motion Mr. Ludlam wiſhed to avoid.

The ſc[illegible] which are acted on by the crutch do not preſerve a conſtant diſtance,

Another great fault of this pendulum, is, that of putting in ſcrews through the wooden rod, to clip the flat part from the crutch; now in changes of weather from moiſt to dry, or by heat and cold, theſe ſcrews will accordingly be found, ſometimes to pinch the flat part of the crutch, and at other times, to leave it at liberty, or even to allow it to have a conſiderable degree of ſhake between the ſcrew points: hence will ariſe very different degrees of impulſe communicated to the pendulum ball. Wood has a very ſenſible la-

titudinary

becauſe wood varies acroſs the grain by temperature and moiſture, &c.

titudinary alteration, by the effects of heat and cold, or by dry and moiſt, yet theſe effects on its length are, rather but very imperceptible, or at leaſt, are, in ſo ſmall a degree, as have not been well aſcertained, to what extent they are; even by thoſe who have made experiments with it on the pyrometer.

The end expanſion of wood yet unknown.

Mr. Berthoud condemns wood as being unfit for pendulum rods, and although he, and others have given tables of the expanſion of various materials, yet none of them have condeſcended to ſay, what were the effects of heat and cold on wood of any ſort.

Wooden pendulums ſeem equal to the compound pendulums,

I am well convinced, that a pendulum may be ſo fitted up with a wooden rod, as to perform with ſuch a degree of correctneſs, that it would be a very difficult matter to ſay, whether it, or the beſt compenſation pendulum yet conſtructed, when both comparatively tried, was the neareſt to accurate meaſuring of time.

but are often ill conſtructed.

There has been one circumſtance attending all thoſe pendulums fitted up with a wooden rod, that their errors have been imputed to the rod, when in fact, they ought to have been imputed to that of the ball, and theſe errors have ariſen from the manner by which the ball is hung on the rod, reſting on its lower edge on the regulating nut; and lead having a conſiderable degree of expanſion, clocks having ſuch pendulums have been found, by thoſe who attended properly to their going, to have gone conſtantly faſter in ſummer than in winter. Let the ball therefore be hung by its centre on the rod, and a much greater degree of accuracy in time-keeping will be ſeen to follow.

Improved pendulum.

In conſequence of my trials with Mr. Ludlam's pendulums, they were found to be extremely troubleſome to put on beat, from their ſtrong tendency to this gyratory ſort of motion, it being ſome while, before they would come to move ſteadily; I hit not only on a method of putting a clock, as it were mechanically on beat, (the common way being by the ear) but was led to think on a way of conſtructing a pendulum, in which this gyratory motion could hardly take place, even although the pendulum ſhould be but indifferently fitted up. This laſt was by following a method quite the reverſe to that of Mr. Ludlam's, in making the pendulum ball, which I made in the uſual or lenticular form, and in order that it ſhould have as much of its matter preſerved at the centre, there were two ſteel wires put through the ball, paſſing parallel to each other, and each put a little to one ſide of the centre, through which pendulum rods are uſually made to paſs, and when the rod is wood, it neceſſarily takes away much of the matter from the centre of the ball.

A pendulum of this ſort of mine, and which has a degree of compenſation in it, I made to a clock, which my brother got, and which he has at his houſe, No. 31, Roſamon Street, Clerkenwell.†

The experiment applied to watches, &c.

I mentioned this mode of putting clocks mechanically on beat, to my ingenious friend, Mr. Pennington, who has ſince very happily applied the ſame ſucceſsfully in his practice to watches, &c.

Pendulum for regulating the ſtriking part of clocks; not new.

I ſee you have mentioned in your Journal of July laſt, the application of a pendulum to regulate the ſtriking part of clocks, from the Society for encouraging Arts, &c. having given a premium for it to Mr. Maſſy.

This is not a new thing. Mr. Berthoud mentions it as his invention, and you will ſee a drawing, and the deſcription of it, in his *Eſſai ſur l'Horlogerie*, publiſhed in 1763. Julien Le Roy, in my humble opinion, is certainly intitled to the merit of it, as it appears to me, that Berthoud has taken the idea of it from Le Roy's method of regulating the ſtriking train of his repeating watches, which he *invented*, and applied to them about the year 1754.

The crank ſcapement, not new;

There was a premium given alſo in 1799, by the honourable ſociety above mentioned, for a new 'ſcapement by Mr. Goodrich*; now this 'ſcapement was made prior to the year 1740, and invented by the Abbé Soumille; and another nearly of the ſame ſort was made at Rome before that period, as may be ſeen in the collection publiſhed by Thiout, in the year 1741. Surely nobody would think of adopting ſuch a 'ſcapement as this, whoſe principle ſeems to be that of depriving the pendulum of the moſt valuable property it poſſeſſes, viz. that of having the liberty to operate freely under the influence of gravitation. This 'ſcapement keeping the pendulum, as it were in leading ſtrings.

and bad.

I am ſurpriſed that none of the members of this honourable and uſeful ſociety, ſhould not have known, that theſe things for which premiums have been adjudged were not new, perhaps they were nevertheleſs *ſo*, to thoſe to whom the premiums were adjudged.

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† See Philoſ. Journal, quarto ſeries, III. 342, 416. It is crank.—N.

Among Thiout's collection may be ſeen a 'ſcapement, which he gives to the ingenious Dutertre, about the year 1724. Peter Le Roy gave into the Royal Academy of Arts and Sciences in the year 1727, a deſcription of the ſame 'ſcapement which Dutertre claimed, or pretended to ſay was his invention. The miſtake lays with Thiout, for Dutertre's 'ſcapement is an improvement of a very old one, uſed by the Germans in large clocks, perhaps long before the year 1600, though neither the author of it, nor the time *when*, can now be traced.

A 'ſcapement made long ago

However it was before Dr. Hook's time, who invented one of the ſame kind before 1658. I would not have entered ſo minutely into this diſcuſſion, but to ſhow the progreſs of the duplex, which in its preſent form, was firſt made ſo by Peter Le Roy, who afterwards gave it up for a bad one; yet it ought to be allowed that that of Dutertre's muſt have led him very eaſily to it. So much for the duplex 'ſcapement, ſo called by the workmen, and now in ſuch general uſe. A celebrated Philoſopher in the ſupplement to the Edinburgh Encyclopedia, under the article watch work, has given it a French ſurname, that of Dupleix, for what reaſon, I know not.

leading to the duplex 'ſcapement which was firſt made by Peter Le Roy.

Thiout has given alſo among the number, a 'ſcapement of his own, a ſort of detached one, and which may be conſidered as the foundation of the detached one of the preſent day, now ſo much improved, and of ſuch general uſe in all our pocket and box chronometers: indeed it ſeems a ſcapement indiſpenſably neceſſary for theſe purpoſes. Yet Berthoud in his famous time-keeper, No. 8, uſed a very different one from that of the detached ſort.

The origin of the common detached 'ſcapement aſcribed to Thiout.

I am,

Sir, with much eſteem, Your's,

THOMAS REID.

Edinburgh, 25*th March*, 1805.

II.

An Analyſis of the Magnetical Pyrites; with Remarks on ſome of the other Sulphurets of Iron. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

(Concluded from page 276.)

§ VII.

Whether the artificial pyrites with minimum of ſulphur be, like the natural, magnetical.

SO far, therefore, as can be proved by ſimilarity in chemical properties and analyſis, the magnetical pyrites is indiſputably a natural ſulphuret, completely the ſame with that which till now has been only known as an artificial product; but, that the mind may be perfectly ſatisfied, another queſtion muſt be ſolved, namely, how far do they accord in receiving and retaining the property of magnetiſm? common pyrites do not appear to affect the magnetic needle, or, if ſome of them ſlightly act by attraction, (which however I never could perceive, nor recollect to have read in works expreſsly relating to magnetiſm,) yet they do not poſſeſs, nor appear capable of acquiring, any magnetic polarity. As, therefore, the iron of pyrites is undoubtedly in the metallic ſtate, and in a conſiderable proportion, the deſtruction of this characteriſtic property of metallic iron, muſt be aſcribed to the other ingredient, ſulphur.

The artificial compound is not a mere mixture.

But we have lately ſeen, that a natural combination of iron with 36.50 or 37 *per cent.* of ſulphur, is in poſſeſſion of all the properties ſuppoſed hitherto to appertain (in any marked degree) almoſt excluſively to the well known magnetic iron ore; and that the combination alluded to is ſtrictly chemical, and not (as at firſt might have been imagined) a mixture of particles of magnetic iron ore with common pyrites*.

The compound directly formed at red heat.

This is certainly very remarkable; and it induced me to examine the effects produced by ſulphur, on the capacity of metallic iron for receiving and retaining the magnetic properties. I therefore prepared ſome ſulphuret of iron, by adding a large quantity of ſulphur to fine iron wire, in a moderate red heat.

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* This has been ſufficiently proved, by the facts which have been ſtated; I ſhall however add, that upon digeſting a mixture of the powder of common pyrites and iron filings in muriatic acid, I only obtained hydrogen gas, exactly as if I had employed the iron filings without the pyrites.

The

Reſembled common pyrites; but was capable of magnetiſm.

The internal colour and luſtre of the product, were not very unlike thoſe of the magnetical pyrites; and, after the maſs had been placed during a few hours between magnetical bars, I found that it poſſeſſed ſo ſtrong a degree of polarity, as to attract or repel the needle completely round upon its pivot; and although ſeveral weeks have elapſed ſince it has been removed from the magnetical bars, it ſtill retains its power, with little diminution; like the magnetical pyrites, however, in its natural ſtate, it is not ſufficiently powerful to attract and take up iron filings.

It contained leſs ſulphur than the magnetical pyrites. More ſulphur added and low ignition.

But this ſulphuret did not contain ſo much ſulphur as the magnetical pyrites; I therefore mixed ſome of it, reduced to powder, with a large quantity of ſulphur, and ſubjected it to diſtillation in a retort, which was at length heated until the intire bulb became red.

It was attractable by the magnet.

The ſulphuret, by this operation, had aſſumed very much the appearance of the powder of common pyrites, in reſpect to colour; but, in its chemical properties, ſuch as ſolubility in muriatic acid, with the production of ſulphuretted hydrogen gas, as well as in the nature of the precipitates it afforded with pruſſiate of potaſh and with ammonia, it perfectly reſembled the magnetical pyrites. Moreover, by analyſis, it was found to conſiſt of 35 parts of ſulphur and 65 of iron; and although (being in a pulverulent ſtate) its power, as to receiving and retaining the magnetic property, could not ſo eaſily be examined, yet, by being powerfully attracted by the magnet, with ſome other circumſtances, there was every reaſon to conclude, that in this reſpect alſo it was not inferior.

Another proportion of ſulphuret was formed, as above deſcribed; it was placed between magnetical bars, and, in like manner, received and retained the magnetic power.

Hence 36 or 37 per cent. does not prevent iron from becoming a magnet.

It is certain, therefore, that when a quantity of ſulphur equal to 35 or 37 *per cent.* is combined with iron, it not only does not prevent the iron from receiving the magnetic fluid, but enables it to retain it, ſo that the maſs acts in every reſpect as a permanent magnet.

Black oxide of iron combines with ſulphur leſs readily.

Black oxide of iron, by one operation, does not appear to combine with ſulphur ſo readily as iron filings; a ſecond operation, however, converts it into a ſulphuret, very much reſembling that which has juſt been deſcribed, including the chemical as well as the magnetical properties; but, undoubtedly, by theſe proceſſes, it is progreſſively converted, perfectly or very nearly, into the metallic ſtate.

Iron combined with a larger proportion of oxygen, ſuch as the fine gray ſpecular iron from Sweden, will not form a ſulphuret by the direct application of ſulphur, in one operation; although it becomes of a dark brown colour, partly irideſcent, and is moderately attracted by a magnet.

Magnetical pyrites combined with 9 *per cent.* more of ſulphur,

Fifty grains of the magnetical pyrites, reduced to powder, and mixed with three times the weight of ſulphur, were diſtilled in a retort, until the bulb became moderately red-hot. After the diſtillation, the pyrites weighed 54.50; conſequently, the addition of ſulphur was 9 *per cent.* making the total = 45.50 or 46 *per cent.* The powder was become greeniſh-yellow, very like that of the common pyrites: it did not afford any ſulphuretted hydrogen, when digeſted in muriatic acid; but it nevertheleſs was partially diſſolved, and the ſolution, when examined by pruſſiate of potaſh, and by ammonia, was not different from that of the crude magnetical pyrites.

—was ſtill attracted by the magnet.

The powder which had been diſtilled with ſulphur, and which had thus received an addition of 9 *per cent.* to its original quantity, *was ſtill capable of being completely taken up by a magnet.*

Iron ceaſes to be acted on by the magnet when the doſe of ſulphur is at ſome point between 46 and 52 *per cent.*

From the whole of the experiments which have been related, it is therefore evident, that iron, when combined with a conſiderable proportion of ſulphur, is not only ſtill capable of receiving the magnetic property, but is alſo thereby enabled to retain it, and thus (as I have already remarked) becomes a complete magnet; and it is not a little curious, that iron combined (as above ſtated) with 45 or 46 *per cent.* of ſulphur, is capable of being taken up by a magnet, whilſt iron combined with 52 *per cent.* or more, of ſulphur, (although likewiſe in the metallic ſtate,) does not ſenſibly affect the magnetic needle; and hence, ſmall as the difference may appear, there is reaſon to conclude, that the capacity of iron for magnetic action is deſtroyed by a certain proportion of ſulphur, the effects of which, although little if at all ſenſible at 46 *per cent.* are yet nearly or quite abſolute, in this deſtruction of magnetic influence, before it amounts to 52. But, what the exact intermediate proportion of ſulphur may be, which is adequate to produce this effect, I have not as yet determined by actual experiment.

Sulphur acts on iron like carbon in cauſing it to contain magnetiſm,

As carbon acts on ſoft iron, (which, although it moſt readily receives the magnetic influence, is unable to retain it ſo as to become a magnet, without the addition of a certain proportion of carbon, by which it is rendered hard and brittle, or, in other words, is converted into ſteel, ſo, in like manner, does ſulphur ſeem to act; for it has been proved, by the preceding experiments, that the brittle maſs formed by the union of a certain proportion of this ſubſtance with iron, whether by nature or by art, becomes capable of retaining the magnetic virtue, and of acting as a complete magnet.

—and ſo likewiſe does phoſphorus.

This remarkable coincidence, in the effects produced on iron by carbon and ſulphur, induced me to try the effects of phoſphorus; and my hope of ſucceſs was increaſed by the remark of Mr. Pelletier, who ſays, that "the phoſphuret of iron is attracted by the magnet;"[*] and therefore, although certain bodies may be thus attracted, without being capable of actually becoming permanent magnets, I was deſirous to examine what might be the power, in this reſpect, of phoſphuret of iron.

Experiment.

Phoſphuret of iron was made;

I therefore prepared a quantity of phoſphuret of iron, in the direct way, *viz.* by adding phoſphorus, cut into ſmall pieces, to fine iron wire made moderately red-hot in a crucible. The uſual phenomena took place, ſuch as the brilliant white flame, and the rapid melting of the iron, which, when cold, was white, with a ſtriated grain, extremely brittle, hard, and completely converted into a phoſphuret. The fragments of this were powerfully attracted by a magnet; and, after I had placed two or three of the largeſt pieces, during a few hours, between magnetical bars, I had the pleaſure to find that theſe had become powerful magnets, which not only attracted or repelled the needle completely round, but were able to take up iron filings, and ſmall pieces, about half an inch in length, of fine harpſichord wire; and, although they have now been removed from the magnetical bars more than three weeks, I cannot diſcover any diminution of the power which had thus been communicated to them.

—and by the touch formed a powerful magnet.

The three inflammable ſubſtances, *carbon*, *ſulphur*, and *phoſphorus*, which, by their chemical effects on iron, in many reſpects reſemble each other, have now therefore been proved alike to poſſeſs the property of enabling iron to retain the power of magnetiſm; but I ſhall conſider this more fully in the following ſection.

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[*] "Le Phoſphure de Fer eſt attirable a l'aimant." Annales de Chimie, Tome XIII. p. 114.

§ VIII.

From the whole which has been ſtated we find,

General reſults. Magnetic pyrites is a Britiſh mineral,

1. That the ſubſtance called magnetical pyrites, which has hitherto been found only in Saxony and a few other places, is alſo a Britiſh mineral, and that, in Caernarvonſhire, it forms a vein of conſiderable extent, breadth, and depth.

—containing about 37 ſulphur and 63 iron.

2. That the component ingredients of it are ſulphur and metallic iron; the former being in the proportion of 36.50 or 37, and the latter about 63.50 or 63.

It differs in its properties from common pyrites which contains more ſulphur.

3. That the chemical and other properties of this ſubſtance are very different from thoſe of the common martial pyrites, which however are alſo compoſed of ſulphur and iron, varying in proportion, from 52.15 to 54.34 of ſulphur, and from 47.85 to 45.66 of metallic iron; the difference between the common pyrites which were examined being therefore 2.19, and the mean proportions amounting to 53.24 of ſulphur, and 46.75 of iron; conſequently, the difference between the relative proportions, in the compoſition of the magnetical pyrites and of the common pyrites, is nearly 16.74, or 16.24.

It is identically the ſame as the artificial ſulphuret.

4. That, as the magnetical pyrites agrees in analytical reſults, as well as in all chemical and other properties, with that ſulphuret of iron which hitherto has been only known as an artificial product, there is no doubt but that it is identically the ſame; and we may conclude, that its proportions are moſt probably ſubjected to a certain law, (as Mr. Prouſt has obſerved in the caſe of the artificial ſulphuret,) which law, under certain circumſtances, and eſpecially during the natural formation of this ſubſtance in the humid way, may be ſuppoſed to act in an almoſt invariable manner.

In common pyrites the ſulphur predominates.

5. That, in the formation of common martial pyrites, there is a deviation from this law, and that ſulphur becomes the predominant ingredient, which is variable in quantity, but which, by the preſent experiments, has not been found to exceed 54.34 *per cent.* a proportion, however, that poſſibly may be ſurpaſſed in other pyrites, which have not as yet been chemically examined.

6. That iron, when combined naturally or artificially with 36.50 or 37 of ſulphur, is not only ſtill capable of receiving the magnetic fluid, but is alſo rendered capable of retaining it, ſo as to become in every reſpect a permanent magnet; and the ſame may, in a great meaſure, be inferred reſpecting iron which has been artificially combined with 45.50 *per cent.* of ſulphur.

Limits of the ſulphur in magnetic pyrites.

7. That beyond this proportion of 45.50 or 46 *per cent.* of ſulphur, (in the natural common pyrites,) all ſuſceptibility of the magnetic influence appears to be deſtroyed; and, although the preciſe proportion which is capable of producing this effect, has not yet been determined by actual experiment, it is certain that the limits are between 45.50 and 52.15; unleſs ſome unknown alteration has taken place in the ſtate of the ſulphur, or of the iron in the common martial pyrites.

Proportion beyond which the magnetic influence is loſt.

8. That, as carbon, when combined in a certain proportion with iron, (forming ſteel,) enables it to become a permanent magnet, and as a certain proportion of ſulphur communicates the ſame quality to iron, ſo alſo were found to be the effects of phoſphorus; for the phoſphuret of iron, in this reſpect, was by much the moſt powerful, at leaſt when conſidered comparatively with ſulphuret of iron.

As carbon renders iron tenacious of magnetiſm, ſo alſo do ſulphur and phoſphorus.

9. And laſtly, that as carbon, ſulphur, and phoſphorus, produce, by their union with iron, many chemical effects of much ſimilarity, ſo do each of them, when combined with that metal in certain proportions, not only permit it to receive, but alſo give it the peculiar power of retaining, the magnetical properties; and thus, henceforth, in addition to that carburet of iron called ſteel, certain ſulphurets and phoſphurets of iron may be regarded as bodies peculiarly ſuſceptible of ſtrong magnetical impregnation.

Concluſion.

Having thus, for the greater perſpicuity, reduced the principal facts of this Paper into a conciſe order, I ſhall now make ſome general obſervations.

It is undoubtedly not a little ſingular, that a ſubſtance like the magnetical pyrites, which, although not common, has been long known to mineralogiſts, ſhould not hitherto have been chemically examined, eſpecially as mineralogical authors have mentioned the analyſis of it as a deſideratum. The reſult of this which I have attempted, proves that it is really deſerving of notice; for thus we have aſcertained, that the ſulphuret of iron hitherto known only as an artificial product, is alſo formed

Remarks.

The magnetical pyrites is an intereſting product.

by nature; and that the compoſition of this laſt, agrees with thoſe proportions of the artificial ſulphuret which have been ſtated by Mr. Prouſt.

No intermediate natural product between the common and the mag. pyrites.

But, from this ſulphuret or magnetical pyrites, I have not, by analyſis, as yet been able to diſcover any regular or immediate gradations into the common pyrites; for the leaſt proportion of ſulphur in theſe amounted to 52.15, and the greateſt proportion to 54.34; ſo that, between the magnetical and the common pyrites, the difference is conſiderable, in the proportions of their component ſubſtances, as well as in their phyſical and chemical properties; whilſt the difference which I have hitherto been able to detect in the proportions of ſome of the common pyrites (very diſſimilar in figure, luſtre, colour, and hardneſs,) has only amounted to 2.19.

Remarks on Prouſt's experiments.

Mr. Prouſt, in a general way, conſiders common pyrites to differ from the firſt ſulphuret, or that compoſed of 60 parts of ſulphur and 100 of iron, (= 37.50 *per cent.*) by containing a farther addition of half the above quantity of ſulphur, or 90 parts of ſulphur and 100 of iron, (= 47.36 *per cent.*) but this opinion he appears to have formed, in conſequence of reſults obtained by ſynthetical experiments made in the dry way.—Now, when we conſider how difficult it is to regulate the high degrees of temperature, and what a numerous chain of alterations in the relative order of affinities moſt commonly reſult from alterations in theſe degrees of heat, it ſeems to me that we cannot rely, with abſolute certainty, on ſynthetical experiments made in the above way, unleſs they are corrected, and contraſted with analytical experiments made on the ſame ſubſtances. But it does not appear, from the two memoirs publiſhed by Mr. Prouſt, to which I have ſo frequently alluded, that that gentleman did more, in reſpect to analyſis, than diſtil the cubic and dodecaedral pyrites found near Soria, from which he obtained about 20 *per cent.* of ſulphur; and, having obſerved that the reſiduum poſſeſſed the properties of the ſulphuret which has been commonly prepared in laboratories, he concluded that the ſulphur obtained from the pyrites, is the exceſs of that proportion which is requiſite to form the ſulphuret, the proportions of which, therefore, he by ſyntheſis aſcertained to be, as I have above ſtated, = 37.50 of ſulphur, and 62.50 of iron, or 60 of ſulphur combined with 100 of iron; and laſtly, having formed 318 grains of this ſulphuret from 200 grains of iron filings, he diſtilled the ſulphuret with an additional quantity of ſulphur, in an inferior degree of heat, and obtained 378 grains of a ſubſtance which, excepting denſity, was ſimilar to the common martial pyrites.*

It is however to be regretted, that Mr. Prouſt did not make a regular analyſis of the pyrites of Soria, and of the reſiduum after diſtillation; for (unleſs theſe pyrites are very different from thoſe which I have examined) he would moſt probably have found the proportion of ſulphur greater than that which he has aſſigned to natural pyrites in general. This at leaſt there is great reaſon to ſuppoſe, if we allow that moſt or all of the pyrites have been formed in the humid way, by which, we may conceive, a larger proportion of ſulphur may be introduced into the compound, than can take place in high degrees of temperature. And this opinion is corroborated by the reſults of my analyſes; for, inſtead of finding the general proportions to be 47.36 of ſulphur and 52.64 of iron, the mean reſult of theſe analyſes is very nearly the reverſe, being 53.24 of ſulphur and 46.76 of iron.

He did not make a regular analyſis.

Mr. Prouſt is alſo of opinion, that the pyrites which contain the ſmalleſt quantity of ſulphur, are thoſe which are moſt liable to vitriolization; and, on the contrary, that thoſe which contain the largeſt proportion, are the leaſt affected by the air or weather.† This opinion of the learned profeſſor, by no means accords with ſuch obſervations as I have been able to make; for the cubic, dodecaedral, and other regularly cryſtallized pyrites, are liable to oxidizement, ſo as to become what are called hepatic iron ores, but not to vitriolization; whilſt the radiated pyrites (at leaſt thoſe of this country) are by much the moſt ſubject to the latter effect; and therefore, as the reſults of the preceding analyſes ſhow that the cryſtallized pyrites contain leſs ſulphur than the radiated pyrites, I might be induced to adopt the contrary opinion. But I am inclined to attribute the effect of vitriolization obſerved in ſome of the pyrites, not ſo much to the proportion, as to the ſtate of the ſulphur in the compound; for I much ſuſpect, that a prediſpoſition to vitriolization, in theſe pyrites, is produced by a ſmall portion of oxygen being previouſly combined with a part, or with the general maſs,

Prouſt apprehends that pyrites holding the leaſt ſulphur are moſt liable to vitriolization.

But this diſpoſition moſt probably ariſes from a commencement of oxidation.

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* *Journal de Phyſique*, Tome LIV. p. 92.

† *Journal de Phyſique*, Tome LIII. p. 94.

of

of the ſulphur, at the time of the original formation of theſe ſubſtances, ſo that the ſtate of the ſulphur is tending to that of oxide, and thus the acceſſion of a farther addition of oxygen becomes facilitated. We have an example of ſimilar effects in phoſphorus, when (as is commonly ſaid) it is half burned, for the purpoſe of preparing the phoſphorus bottles; and the propenſity to vitriolization, obſerved in many of the half-roaſted ſulphureous ores, appears to me to ariſe from this cauſe, rather than from the mere diminution of the original proportion of ſulphur, or the actual immediate converſion of part of it into ſulphuric acid; nevertheleſs, I offer this opinion, at preſent, only as a probable conjecture, which may be inveſtigated by future experiments and obſervations.

The magnetical properties of the ſulphuret is a remarkable fact,

The magnetical properties of the ſulphuret of iron which forms the principal ſubject of this Paper, muſt be regarded as a remarkable fact; for I have not found, in the various publications on magnetiſm which I have had the means of conſulting, even the moſt remote hint, that iron when combined with ſulphur, is poſſeſſed of the power of receiving and retaining the magnetic fluid; and, judging by the properties of common pyrites, we might have ſuppoſed that ſulphur annihilated this power in iron, as indeed ſeems to have been the opinion of mineralogiſts, who have never enumerated magnetical attraction amongſt the phyſical properties of thoſe bodies; and, although Werner, Widenmann, Emmerling, and Brochant, have arranged the magnetical pyrites with the ſulphurets of iron, yet the magnetical property could not with certainty be ſtated as inherent in the ſulphuret, for, at that time, this ſubſtance had not been ſubjected to a regular chemical analyſis, and the magnetical property might therefore be ſuſpected to ariſe from interſperſed particles of the common magnetical iron ore. This probably has been the opinion of the Abbé Haüy; for, in his extenſive Treatiſe on Mineralogy lately publiſhed, I cannot find any mention made of the magnetical pyrites, either amongſt the ſulphurets or amongſt the other ores of iron.

little or ſcarcely noticed.

The magnet ſaid to conſiſt of iron with 10 to 20 oxigen.

"In the mineral kingdom, a great variety of ſubſtances, and even ſome of the gems, exert a feeble degree of attraction on the magnetic needle, and ſometimes alſo acquire a ſlight degree of polarity;* but, as this wonderful property has only been obſerved conſpicuouſly powerful in one ſpecies of iron ore, this has been always emphatically called *the Magnet*,* and is ſaid to conſiſt of metallic iron combined with from 10 to 20 *per cent.* of oxygen.

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* Cavallo on Magnetiſm, page 73.

The ſulphuret is a different kind of magnet.

From the facts, however, which have been recently ſtated, we now find that there is another natural ſubſtance, apparently very different from the magnet in chemical compoſition, but nevertheleſs approaching very nearly to it in power, which is found in ſeveral parts of our globe, and particularly in a province of this kingdom, where it conſtitutes a vein; running north and ſouth, of a conſiderable extent, and ſeveral yards in width and thickneſs.

The phoſphuret magnet.

From the experiments alſo, which have been made on the artificial preparation of this ſubſtance, we find, that it is capable of receiving the magnetic properties when the proportion of ſulphur amounts to 37 *per cent.* and is ſtill powerfully attracted when a much larger quantity of ſulphur is preſent. There is, however, ſome point at which all theſe effects ceaſe, and this point appears to be, when the ſulphur is in ſome proportion between 45 or 46 and 52 *per cent.* The preceding experiments have alſo proved, that iron when combined with phoſphorus, likewiſe poſſeſſes the power of becoming a magnet to a very remarkable degree; and, by the ſimilarity, in this reſpect, of the carburet of iron called ſteel, to the above ſulphuret and phoſphuret, a very remarkable analogy is eſtabliſhed between the effects produced on iron, by carbon, ſulphur, and phoſphorus.

Of carburets, plumbago is not magnetical: other ſteel compounds form powerful magnets.

Carbon, when combined in a very large proportion with iron, forms the carburet of that metal called plumbago; a brittle ſubſtance, inſoluble in muriatic acid, and deſtitute of magnetical properties. But, ſmaller proportions of carbon, with the ſame metal, conſtitute the various carburets included between black caſt iron and ſoft caſt ſteel;† bodies which are more or leſs brittle, ſoluble in muriatic acid, and more or leſs ſuſceptible of magnetical impregnation; ſome of them form the moſt powerful magnets hitherto diſcovered.

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* In a future Paper, it is my intention to give an account of ſome comparative analyſes of the varieties of this ſubſtance.

† "When the carbon exceeds, the compound is carburet of iron "or plumbago: when the iron exceeds, the compound is ſteel, or "caſt iron, in various ſtates, according to the proportion. All "theſe compounds may be conſidered as ſubcarburets of iron."—Thomſon's Syſtem of Chemiſtry, Vol. I. p. 165.

Mr.

Sulphur and iron have ſimilar habitudes.

Sulphur, in like manner, combines with iron in a large proportion, forming the common pyrites, which are brittle, almoſt or quite inſoluble in muriatic acid, and devoid of magnetical properties. Sulphur in ſmaller proportions, forms ſulphurets which are alſo brittle, but are ſoluble in muriatic acid, and ſtrongly ſuſceptible of magnetical impregnation.

So likewiſe phoſphorus and iron.

Phoſphorus alſo, when combined with iron, makes it brittle, and enables it powerfully to receive and retain the magnetical properties; ſo that, conſidering the great ſimilarity which prevails in other reſpects, it may not ſeem raſh to conclude, that phoſphorus (like carbon and ſulphur,) when combined with iron in a very large proportion, may form a ſubſtance incapable of becoming magnetical, although, in ſmaller proportion, (as we have ſeen,) it conſtitutes compounds which are not only capable of receiving, but alſo of retaining, the magnetical properties, even ſo far as, in ſome caſes, to ſeem likely to form magnets of great power; and, ſpeaking generally of the carburets, ſulphurets, and phoſphurets of iron, I have no doubt but that, by accurate experiments, we ſhall find that a certain proportion of the ingredients of each, conſtitutes a maximum in the magnetical power of theſe three bodies. When this maximum has been aſcertained, it would be proper to compare the relative magnetical power of ſteel (which hitherto has alone been em-

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Mr. Muſhet, in the following Table, exhibits the proportion of charcoal which diſappeared, during the converſion of iron to the different varieties of ſubcarburet known in commerce.

“ Charcoal abſorbed.	Reſult.
$\frac{1}{120}$ - -	Soft caſt ſteel.
$\frac{1}{100}$ - -	Common caſt ſteel.
$\frac{1}{90}$ - -	The ſame, but harder.
$\frac{1}{50}$ - -	The ſame, too hard for drawing.
$\frac{1}{25}$ - -	White caſt iron.
$\frac{1}{20}$ - -	Mottled caſt iron.
$\frac{1}{15}$ - - -	Black caſt iron.

“ When the carbon amounts to about $\frac{1}{60}$ of the whole maſs, the “ hardneſs is at the maximum.” Thomſon, Vol. I. p. 166; and Phil. Magazine, Vol. XIII. pp. 142 and 148.

ployed to form artificial magnets) with that of ſulphuret and phoſphuret of iron; each being firſt examined in the form of a ſingle maſs or bar of equal weight, and afterwards in the ſtate of compound magnets, formed like the large horſe-ſhoe magnets, by the ſeparate arrangement of an equal number of bars of the ſame ſubſtance in a box of braſs.

An extenſive field for experiments of intereſt and importance.

The effects of the above compound magnets ſhould then be tried againſt others, compoſed of bars of the three different ſubſtances, various in number, and in the mode of arrangement; and, laſtly, it would be intereſting to make a ſeries of experiments on chemical compounds, formed by uniting different proportions of carbon, ſulphur, and phoſphorus, with one and the ſame maſs of iron. Theſe quadruple compounds, which, according to the modern chemical nomenclature, may be called carburo-ſulphuro phoſphurets, or phoſphuro-ſulphuro-carburets, &c. of iron, are as yet unknown as to their chemical properties, and may alſo, by the inveſtigation of their magnetical properties, afford ſome curious reſults. At any rate, an unexplored field of extenſive reſearch appears to be opened, which poſſibly may furniſh important additions to the hiſtory of magnetiſm, a branch of ſcience which of late years has been but little augmented, and which, amidſt the preſent rapid progreſs of human knowledge, remains immerſed in conſiderable obſcurity.

III.

Extract of a Memoir of Mr. ERMAN, *entitled Obſervations and Doubts concerning Atmoſpheric Electricity.*

(Concluded from p. 300, Vol. X.)

Two electrometers which do not indicate divergence alone, ſhew it when one is paſſed above the other.

AN electrometer furniſhed with a rod three feet in length, and placed in the open air, does not ſhew any divergence; but when the bent point of another electrometer, which alſo exhibits no divergence, is moved above the firſt, and even when the motion is parallel to the horizon, the leaves of the latter will be ſeen to diverge negatively, without the ſecond giving any ſign of electricity.

This effect probably ariſes from the diviſion of the electricity.

It is very probable that the effect of theſe vapourous and aqueous meteorological maſſes is manifeſted at the points of the electrometer, by this ſame action, which depends on the diviſion of the electricity, and this explains the ſudden changes which ſupervene in the poſitive or negative electric ſtate of the earth: it is not even probable that the clouds poſſeſs in themſelves a negative electricity, or that the vapours of water ſhould be always in this ſtate. Mr. Erman thinks he can prove that the vapours of water are only negative when the body from which they proceed is inſulated; but that they become poſitive as ſoon as it is brought into contact with the earth. Rain, after its fall, leaves bodies in the ſtate of *minus* electricity, which is agreeable to the preceding experiments; ſnow produces this effect ſo much the better, as it leaves the ſurrounding air in a ſtate of dryneſs, which augments its inſulating properties.

The poſitive or negative ſtate of aqueous vapour depends on inſulation.

Influence of the clouds on the earth.

The clouds which have a tendency to rain or ſnow muſt neceſſarily produce their influence on the ground, and it is for this reaſon that the oppoſition of the cloud and the ground is ſo quickly manifeſted. It would be intereſting to explain the complication ariſing from the variations which the electricity of the earth undergoes, as well with reſpect to its ſpecies as to its intenſity, and Mr. Erman is at preſent engaged in this ſubject.

The different phenomena of the divergence of an inſulated point explained by the ſame law.

With reſpect to the variable degree of the poſitive divergence of an inſulated point fixed in the earth, it may perhaps be attributed to the greater or leſs conducting quality of the ſurrounding air: the point, when it is very well inſulated from the earth, ſhows a certain degree of poſitive charge when the air is inſulated ſo that electricity cannot be communicated to it; it ſhows zero when the conducting power of the air is nearly equal to the rapidity with which the charge is made; and negative when this power is very ſtrong. Theſe phenomena are thus explained by the ſame law.

Probability that there is no free electricity in the atmoſphere.

Mr. Erman has alſo ſuppoſed in his memoir that there is not any quantity of electricity diſengaged in the atmoſphere; but he does not however aſſert that this is ſtrictly the caſe: he has only ſought to draw the attention of philoſophers to this ſubject, and to ſhow that theſe phenomena are equally well explained by the ſphere of activity of the electricity.

Convenience of the inſtruments.

The experiments are very eaſily made, and the electrometers he uſes are very portable; for when the leaves of gold are placed on one ſide of the cylinder, motion cannot occaſion any accident. With reſpect to the ſticks, they may be made of ſeveral pieces, and be ſcrewed together when they are wanted. Such were the inſtruments Mr. Erman took with him over ſeveral hundred leagues; the ſtick was three lines in diameter at one end, and one line at the other. This philoſopher does not yet know what may be the influence of the different thickneſs of the conductor. One of theſe conductors happened to be of braſs, the other of ſteel; it did not appear to him that the difference of the metal occaſioned any in the reſults. But he purpoſes making new enquiries on this ſubject.

Mr. Erman offers his concluſions with great diffidence: he does not pretend to have formed a new theory, but only to ſtate his doubts on the opinion of thoſe philoſophers who have attributed the phenomena hitherto obſerved to a diſengaged electricity in the atmoſphere. He notices ſeveral other intereſting experiments which he is employed in making upon ſmoke, and particularly on electricity in a vacuum. We ſhall haſten to publiſh the reſults of them as ſoon as they come to hand.

IV.

Deſcription of a Compenſation Curb. By Mr. JAMES SCOTT.

To Mr. NICHOLSON.

SIR,

INCLOSED I ſend you a ſketch of an inſtrument I have conſtructed, much wanted in the profeſſion of watchmaking, for the purpoſe of publiſhing in your Philoſophical Journal, which I preſume will be of much benefit to the public, and at the ſame time may prevent any other perſon from claiming it. Introductory letter.

I ſhall call it a compenſation curb. The conſtruction of this inſtrument is for the purpoſe of expanding and contracting in the different temperatures, ſo as to counteract the error which the pendulum-ſpring is liable to by the ſmalleſt variation of heat or cold.

The inventions that have hitherto been put in practice for the ſame purpoſe, I beg leave to make a few remarks on.—The compound balance, when carefully made and adjuſted, is certainly a very complete counteracting expanſion, and will anſwer exceedingly well on board a ſhip, if there be no material difference in the denſity of the air; if otherwiſe, the balance being loaded, will have to encounter a conſiderable deal more friction, and conſequently be impeded in its vibrations: if worn in the pocket, it is alſo liable to error, as exerciſe will alter its diameter. The compound balance has heretofore got the preference, becauſe artiſts have not been able to invent a compenſation curb adjuſtable to the exact expanſion required (which, by many experiments, I have proved the incloſed to be fully competent to); therefore, the plain balance having no projections on the ſurface, muſt certainly have the advantage. As I am not in the habit of expreſſing my ideas to the public, I hope you will have the goodneſs to rectify any errors in the ſtile, and alſo curtail any part which may appear unneceſſary to you.

I remain, Sir,

Your much obliged humble ſervant,

JAMES SCOTT.

39, *Grafton Street*, *Dublin.*

The following is a Deſcription of the Compenſation Curb.

Deſcription of a compenſation curb.

The ſteel index, *Pl. II. Fig* 2, letter A, is for the purpoſe of ſupporting the curb, which is faſtened by a ſcrew and ſteady pin at R, the circle of which at A is turned with a dovetail, as ſhewn at H, and is ſlit ſo as to ſnap into the frame-plate, by which means it may be turned, and will carry the curb, ſo as to regulate the machine in the common way. D and E are two circles compoſed of braſs and ſteel ſoldered together, the outſide of E braſs and the inſide ſteel; but the outſide of D is ſteel and the inſide braſs; ſo that the one circle expands when the other contracts; by which means the acting part of the curb at C will ſhift towards the index with heat, and prevent the vibrations being ſlower, which the expanſion of P, the regulating ſpring and the balance, would otherwiſe occaſion; and on the contrary with cold, it will ſhift its poſition nearer the ſtud I, which the regulating ſpring is pinned to; ſo that let it receive heat or cold, the acting part of the curb at C will at all times keep the regulating ſpring the exact length, to counteract the expanſion of the balance and pendulum-ſpring. V is a piece of ſteel, with a notch cut in it to receive the expanſion circles D and E. F is a ſcrew for faſtening V at any part of the circles; ſo that, by ſtoving the watch, you

Deſcription of a compenſation curb.

you will immediately aſcertain towards what part of the circles you muſt ſhift V. If you find its rate ſlow with heat, you muſt ſhift V to lengthen your circles, and if faſt, the contrary; and by marking your circles each time you have occaſion to ſhift V, yon will be enabled to adjuſt the curb to the exact expanſion required. W is a looſe piece of ſteel, which fits in the notch of V between the two expanſion circles, to keep them faſt in their proper poſitions when ſcrewed by F. S S are two ſteady pins made faſt in the frame-plate, which receive the circle E between them to prevent the action of P, the regulating ſpring, from affecting C, the curb, by moving it to or from the center during the going of the watch. The two expanſion circles require to be made very delicate; it is therefore to be obſerved by the manufacturer of this inſtrument, that the braſs in each circle is to be the thickneſs of the ſteel; ſo that when the two bodies are ſoldered together, they will make two thickneſſes of the edge of a main-ſpring of a watch. The larger your watch will admit the diameter of the curb, the better. It muſt be at leaſt the ſize of the balance.

V.

Letter from Mr. BOSWELL, *in Anſwer to* AN OLD CORRESPONDENT.

To Mr. NICHOLSON.

SIR, *April* 10, 1805.

Mr. Boſwell vindicates himſelf from confidence.

THE perſon ſigning *your Old Correſpondent*, has thrown ſome very undeſerved reflections on my laſt communication in your Journal.

In anſwer to his remarks, I beg leave to obſerve that the introduction to my paper contains ſufficient to juſtify me fully from his imputations.

I have there aſſerted no more than that "I have diſcovered "a method of coming ſo *near the truth*, that ſhould it turn "out to be in reality *not ſo exact* as appears to me, yet it pro"miſes to be ſo uſeful for *common computations* that I am "induced to ſend it for publication, *if you approve of it*."

I know not, Sir, how I could have written in any way farther from "announcing my diſcovery with confidence" than the above, or indeed with more diffidence.

His methods were ſtated as *nearly approaching the truth.*

In the firſt place, I ſtate it only as a *near approach* to the exact truth, which both your and his criticiſms have fully proved it to be: In the next place, I expreſs my doubt of its being even *ſo exact* as it appears to me: And thirdly, I have left its publication to depend *on your approbation.*

If this is not enough to remove all ſuſpicion of confidence, I have to add, that the ſentence which concludes that ſubject in the paper, ſtates, that an unperceived error might ariſe from the ſmallneſs of the circles which I uſed; and beſides this you can teſtify for me, that I wiſhed the paper to be ſuppreſſed altogether before publication, when you ſhewed me that it even wanted an hundredth part of being exact, though with a much greater inaccuracy the matter contained in it would be uſeful for the purpoſes to which I ſtated it might be applied.

As to the proof of the ſecond *fact*, it was not from the "conviction of its obvious accuracy not requiring proof" that I did not inſert any, but becauſe I concluded it muſt be ſufficiently obvious after what I had ſtated of the firſt, that it was the ſame ſort of *experimental* proof I had uſed for both: And if any gentleman will try the experiment as I did, he will find I have not miſſtated the matter.

There may indeed be ſome little impropriety in uſing the word *fact* in a popular ſenſe, in any thing like a mathematical ſtatement; but to notice ſuch a trifle with inverted commas, only appears to indicate a ſpirit of cavilling on the part of your Old Correſpondent.

None of the quadratures of the circle are more than approximations.

But with all his preciſion he has forgot one fact, that puts his computations more on a level with my experiments, which is, that no method has ever yet been diſcovered of computing with *perfect exactneſs* the relative proportions of the circumference to the diameter, and of courſe to the other lines he mentions; and that it is only a far-laboured *approximation* to the truth that has been inſerted in the work from whence he has extracted the proportions which he has uſed; and that therefore what he has "announced with ſo much confidence," (to uſe his own words) is not preciſely demonſtrative truth, but only an approach to it.

I did not ever intend to recommend my method as a perfect and infallible geometrical problem applicable to the more ſublime mathematical ſpeculations, but as a more ready, and, permit me to add, more exact way than that in general uſe, for the humbler purpoſes of common life, ſuch as meaſuring round timber, conduit pipes, engine cylinders, &c.

Mr. B.'s methods have practical utility.

I am, &c.

J. WHITLEY BOSWELL.

VI.

Deſcription of an improved Gate for Fields. By Mr. Charles Waistell *.

Dear Sir,

THE various methods uſed in bracing common gates for fields, prove that not one of them is greatly ſuperior to the reſt; for, if it was, that method would have been generally adopted. Moſt gates are loaded with ſuperfluous timber in ſome of their parts, and are conſtructed upon ſuch bad principles, that they are frequently broken by their own weight, aided by the concuſſion of the head againſt the falling-poſt; and this, long before any part of the wood has begun to decay. I have for ſome time given this ſubject conſiderable attention, being impreſſed with the idea, that if common gates could be conſtructed with leſs timber, and upon better principles, the ſaving of timber only would be of national importance; for we have many millions of gates to uphold in Britain, and their numbers are annually increaſing. The reſult of my labours has been the plan which accompanies this letter. Gates made according to it, poſſeſs great ſtrength, are very light, and of eaſy and ſimple conſtruction. Although uniformity of appearance be not eſſential in a common gate, yet is worth having when it can be obtained, as in this gate, without additional expenſe.

Imperfections of common gates.

Great ſaving in timber if they were made leſs cumberſome.

New plan.

My gate is made with ſhort, and conſequently leſs valuable, oak or aſh timber, than thoſe of the commoneſt conſtruction; its ſtrength is much greater than any other gate made with a like

Account of the author's improvements.

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* Communicated in a letter to Charles Taylor, Eſq. Secretary of the Society of Arts, who returned their thanks for the ſame.

quantity of timber, there being at four diſtant points between the head and the heel, two bars and a brace croſſing each other; and I doubt not that it will be found proportionably more durable: it is, beſides, very eaſy to conſtruct, and requires leſs labour than moſt other common gates. Twenty-nine years ago I deſigned plans for ornamental gates, with ſemi-oval and ſemicircular braces, and had them executed; the plans were ſent to my friends in various diſtant parts of this kingdom, as alſo to Ireland; and I have the pleaſure to obſerve, that they are become almoſt the only ornamental gate in many parts of England. The plans of them I never publiſhed, although they were prepared for engraving fifteen years ago; and I ſhould be as indifferent about my preſent deſign, of a common field gate, if I did not conceive that its publication would materially benefit the public; the introduction of this form being, I conceive, of ſome national importance, as timber has been lately greatly enhanced in price, and is rapidly on the advance.

This gate was deſigned for the approach to a country reſidence; but for common purpoſes, the wicket on one hand, and the ſhort length of rails on the other, may be omitted. I ſhall thank you, if you will have the goodneſs to lay my plan before your reſpectable Society, of which I have, for many years, had the honour to be a member. And ſhould this plan be approved of, I may probably furniſh ſome deſigns for park gates on an improved conſtruction.

I am, Dear Sir,
Your very humble Servant,
CHARLES WAISTELL.

March 22, 1803.

Mr. Charles Taylor.

Reference to the Engraving of Mr. Waiſtell's Gate.

DIMENSIONS.—*(Plate II. Fig. 1.)*

Deſcription and dimenſions of the new conſtructed field-gate.

The heel of the gate to be about	3½	inches ſquare.
The head of ditto - -	2½	by 3 inches.
The top rail or bar - -	3½	by 1½ inches.
The bottom bar - -	3½	by 1¼ inches.
The bar in the middle of the gate	3	by 1¼ inches.
The other bars, and the 4 braces	2½	by 1¼ inches.

Obſervations

Obſervations on its Conſtruction.

Deſcription and dimenſions of the new conſtructed field-gate.

The head and heel of the gate may be of oak, and the bars and braces of fir. Narrow and thick bars, when braced as in this deſign, are ſtronger than broad and thin ones, containing the ſame quantity of timber, and they alſo oppoſe a leſs ſurface to the wind. The two points in the heel of the gate, to which the thimbles are faſtened, may be conſidered as firm or fixed points. From theſe points, viz. 1 and 2, two braces to proceed to 4 and 3, in the middle of the bottom and top bars, and being there ſecured, theſe become fixed points, and from theſe two points, viz. 4 and 3, two braces proceed to 5 and 6, fixing thoſe points. The gate is thus doubly braced, viz. from the top of the heel to the top of the head, by means of the braces 1, 4, and 4, 5; and from the bottom of the heel to the bottom of the head, by means of the braces 2, 3, and 3, 6. On each ſide of the gate are two braces, and thoſe parallel to each other. The brace proceeding from the bottom of the heel of the gate, and that which is parallel to it, as alſo the bottom bar, are all ſtrained in the way of compreſſion, and the brace proceeding from the top of the heel; and the other brace which is parallel to it, and alſo the top bar, are all ſtrained in the way of extenſion. The ſtrains in this gate being none of them tranverſe, but all longitudinal, it would ſupport a vaſt weight at its head without having its form altered. The braces all ſerve the double purpoſe of keeping the gate in its true form, and of ſhortening the bearings of the bars, and ſtrengthening them. Few gates have leſs timber in their braces; and perhaps in no other way can a gate be ſo firmly braced with ſo ſmall a quantity of timber.

At 3, 4, 7, and 8, two braces and a bar of the gate are firmly ſcrewed together by means of iron pins and ſcrew nuts. At the other points, where only one brace croſſes a bar, common gate-nails are uſed.

If, in ſome caſes, a ſtrong top-bar be wanted, to reſiſt the preſſure of heavy cattle, a bar or board, about ſix inches broad, and one inch thick, may be laid with its broad ſide upon the top bas, and fixed thereto by means of the ends of the braces in the middle, and by the heel and head of the gate at the two ends of it. This board will, in this poſition, reſiſt exactly the ſame preſſure as a thick top bar, three inches broad, by four inches deep, although it contain no more than half the timber.

In the ground plan, or horizontal ſection, *Fig.* 7. repreſents a piece of wood, about four inches cube, pinned to the falling poſt, a little below the catch, to ſtop the gate from ſwinging beyond the poſt: another ſtop near the ground may be uſeful.

When gates are hung to open one way only, their heels and heads generally reſt againſt the hanging and falling poſts; but when they are hung according to this deſign, gates may be made about one foot ſhorter for the ſame opening, and conſequently they muſt be lighter, ſtronger, and leſs expenſive.

Of the hanging of Gates.

Hanging of gates.

When the two hooks in the hanging-poſt are placed in the ſame perpendicular line, a gate, like a door, will reſt in any direction in which it may be placed. But, in order that a gate may ſhut itſelf when thrown open, the hooks are not placed exactly perpendicular; the upper hook declining a little towards the falling-poſt, or a few feet beyond it. In whatever direction that hook declines the fartheſt, in the ſame direction will the gate reſt, if unobſtructed, and its head cannot then ſink any lower. Make the head deſcribe half a circle, and it will thus have attained its utmoſt elevation, and will be equally inclined to deſcend either to the right or to the left *.

Particular deſcription of the method of hanging a gate.

The following method of fixing the hooks and thimbles, will, I think, be found to anſwer very well for a gate that is intended to open only one way. Suppoſing the face of the hanging-poſt to be ſet perpendicular, and the upper hook driven in near its inner angle, as is repreſented in the preceding deſign, and that the lower hook muſt be four feet and a half below it; ſuſpend a plumb-line from the upper hook, and at four feet and a half mark the poſt; then at one inch and a half farther from the gateway than this mark, drive in the lower hook; this hook muſt project about half an inch farther from the face of the poſt than the upper hook. In the ſection or ground-plan of the gate,

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* See Chap. II. of Mr. Parker's Eſſay on the Hanging of Gates; and alſo the Agricultural Report for Northumberland, by Meſſrs. Bailey and Culley.

Particular deſcription of the method of hanging a gate.

gate, the two white circles near the hanging-poſt repreſent the places of the two hooks when brought to the ſame horizontal line; that neareſt the gateway repreſents the place of the upper hook. A line drawn through the middle of theſe two circles, and extended each way, will, on one hand, repreſent the gate's natural line of reſt, and, on the other, the line of its higheſt elevation. A gate thus hung will, when thrown open nearly to the line of its higheſt elevation, return to the falling-poſt with a velocity ſufficient to reſiſt a moderately ſtrong wind. This velocity will be either increaſed or diminiſhed, accordingly as the upper hook declines more or leſs from a poſition perpendicular to the lower hook. In order to adapt the thimbles to theſe hooks;—as the lower hook is one inch and a half farther from the gateway than the upper hook, the lower thimble muſt have its eye an inch and a half farther from the heel of the gate than the eye of the upper thimble, in order that the bars of the gate may be in a horizontal poſition when it is ſhut. And, as the upper hook projects half an inch leſs from the hanging-poſt than the lower hook, the upper thimble ſhould be fixed half an inch nearer the farther ſide of the heel of the gate than the lower thimble, in order that the gate may be in a perpendicular poſition when ſhut. If the thimbles have ſtraps embracing the heel of the gate, and proceeding a few inches along each ſide of the bottom and top bars, and if they are fixed to the heel bars and braces, by means of iron pins and ſcrew nuts, great firmneſs will be given to the gate at thoſe two points, which are thoſe that ſuffer the greateſt ſtrains.

⁂ To this communication are annexed a certificate from Mr. Edward Simpſon of Wooden Croft Lodge, near Barnard Caſtle, in favour of the advantage of theſe gates in ſaving and durability; and alſo a letter from Mr. T. N. Parker, author of a well-known Treatiſe on the hanging of Gates, expreſſing his approbation of the ſame.

VII.

Deſcription of an improved Gun-Lock, by Mr. GEORGE DODD.*

Deſcription of an improved gun lock, by reference to the drawings.

THE figures in *Plate* I. repreſent Mr. Dodd's improved gun-lock with its parts in their ſeveral ſituations and poſitions. The ſhaded drawing, *Fig.* 1. repreſents the exterior parts of the lock; and *Fig.* 2. repreſents its interior. *Fig.* 6. exhibits, in perſpective, the tumbler, the ſear and the ſear ſpring in the poſition of whole cock. The outline plans 3, 4 and 5 ſhew the ſeveral poſitions of the parts, at full cock, half cock, and immediately after the diſcharge. The tumbler A terminates on the lower ſide in a tail, as uſual; upon which the main ſpring acts: but, on the upper or oppoſite ſide, it is formed ſo as to have two notches or bents, one very deep for the half cock, and the other ſhallower for the whole cock, as is ſeen in the figures. The circle *a*, *Fig.* 3, ſuppoſed to be deſcribed by the extremity of the bearing face of the tumbler at whole cock, is larger than that through which the extremity of the bearing face of half cock paſſes, ſee *Fig.* 5. and the center of the ſear B is placed in the outer of theſe two circles, having the under ſide of its noſe faſhioned in the arc of the circle *b b*, deſcribed by the motion of its extremity. The bearing ſurfaces of the bents or notches of half and of whole cock are made to fit this face; or, in other words, they form parts of the ſame circle, when reſpectively at whole or half cock. D is the trigger, ſo formed and placed that, at whole cock it trips or draws out the ſear, with great facility and quickneſs, by the action of an inner ſlope or face lying in the direction of a radius of the circle it deſcribes; (ſee *Fig.* 4.) But when at half cock, *Fig.* 3, its action, by means of an outer ſlope or ſurface (which lies intermediate between radii drawn from the centers of the ſear and of the trigger to the inner point of their contact) is ſo far from diſcharging the motion, that it tends to keep the ſear more ſtrongly in its place. Theſe actions and properties are ſufficiently evident from the figures.

Enumeration of the good qualities of this lock.

The advantages of this lock are, 1. It is fully as ſimple in its conſtruction, or rather more ſo, than the common lock, and is therefore no leſs cheap and eaſy to be cleaned by a common ſoldier or workman: 2. It is diſcharged very ſpeedily, and cannot poſſibly catch or hang at the half cock in the act of diſcharging: 3. The bearing parts at half cock are extremely ſtrong and cannot miſs their hold or be thrown out of taking by any accident. In particular the trigger cannot be made to remove the ſear; becauſe its action at half cock is in the contrary direction. Hence it is much more ſimple and its means of ſafety are nearly as ſecure as any bolt, that is to ſay it is perfectly effectual as far as regards the trigger; though it does not, like ſome of the bolt ſtops, prevent the full cock being made. But on the other hand, as the inventor remarks, its ſecurity in no reſpect diminiſhes its ready uſe. For bolts, excluſive of the additional expence, have the diſadvantage of requiring a previous operation before the piece is fit for ſervice. Few people when alarmed have the preſence of mind to unbolt, but they inſtantly attempt to cock. Diſappointment tends to produce that agitation and confuſion of mind which, at ſuch a juncture, may occaſion the loſs of their lives from opponents who are little diſpoſed to ſhew mercy to an enemy from whom they had no reaſon to expect any.

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* From the Tranſactions of the Society of Arts for 1804.

Certificates of the utility and novelty of this invention, from reſpectable makers, with letters of approbation from the Board of Ordnance, were exhibited to the Society of Arts; and it is probable that this apparatus will obtain the encouragement it appears to deſerve. The Society expreſſed their ſentiments by awarding the ſilver medal with the ſum of ten guineas to the inventor.

VIII.

Inveſtigation of the Properties of the Lines drawn in a Circle by Mr. Boswell *in the Tenth Volume of this Journal. By Mr.* John Gough.*

To Mr. NICHOLSON.

SIR, *Middleſhaw, April* 17, 1805.

THE theorems reſpecting the circle, given in your number for March laſt by the ingenious Mr. Boſwell, will undoubtedly prove uſeful to the artiſt and practical philoſopher. On this account

Propoſitions reſpecting the circle's area, &c.

Propoſitions reſpecting the circle's area, &c.

account they ought to be made as correct as poſſible; which has not been as yet done, either by Mr. Boſwell, or by his more ſcientific commentator in your number for the preſent month. This declaration in a manner compels me to undertake the following inveſtigation of the ſubject, in which I ſhall refer to *Fig.* 4. *Plate* XI. of your Journal for April, requeſting the reader to place the letter T at the upper extremity of the vertical diameter, and C at the oppoſite end.

Theorem 1ſt. Let the circle I T F C have unity for its diameter; draw the diameters I F, T C at right angles to each other; biſect the radius T O in W; join I W, and produce it until it meets the circle again in B: theſe things being done, the ſquare upon I B is equal to .8000; which exceeds .7854, or the common expreſſion for the area I T F C by the fraction .0146.

Demonſtration. Put the radius $IO = R = .5$; then $OW = \frac{1}{2}R$; ſince the triangle I O W is right angled at O, by hypotheſis, $IW^2 = R^2 + \frac{1}{4}R^2 = \frac{5R^2}{4}$, *Euc.* 47.1. Now the triangles O I W, B I F are equiangular; becauſe the angle at I is common to both; and the angle F B I is equal to W O I, being right, *Euc.* 31. III; conſequently, as $WI : IO :: IF (= 2IO) : IB$; hence as $WI^2 \left(= \frac{5R^2}{4}\right) : IO^2 (= R^2) :: IF^2 (= 4R^2) : IB^2 \left(= \frac{16R^2}{5}\right)$; but $R^2 = .25$; therefore $IB^2 = \frac{16 \times .25}{5.} = 16 \times .05 = .8000$. But $.8000 - .7854 = .0146$. Q. E. D.

Lemma. The area of the circle I T F C is equal to the rectangle under the radius O I, and the ſemi-circumference I T F; for this area is equal to a triangle having I O for its altitude, and the whole circumference I T F C for its baſe. (*Archimedes de Circulo Prop.* 1ſt.)

Theorem 2nd. If the right line I B be the ſide of a ſquare, which is equal to the area I T F C; and B G be drawn perpendicular to the diameter I F; the ſegment I G of the diameter I F, cut off by B G, is equal to ¼ of the circumference I T F C or the arc T B F.

Demonſtration. The triangle I B F is right angled at B, 31 *E.* III. and B G is perpendicular to I F by hypotheſis

therefore

Propoſitions reſpecting the circle's area, &c.

therefore the rectangle $FI, IG = IB^2$, 8. *E.* VI.; but $IB^2 =$ the rectangle under I O and the arc I F F by lemma; conſequently as $FI \ (= 2IO) : IO :: $ arc I T F: right line I G; 14 *E.* VI. hence $IG = \frac{1}{2}$ the arc I T F, = the arc T B F, Q. E. D.

Corollary 1. If I G, a ſegment of I F be equal to the arc T B F; draw G B perpendicular to I F; and let it meet the circle in B; the line I B is the ſide of a ſquare; which is equal to the area I T F C. This is the converſe of the Theorem.

Cor. 2. If any angle at the centre of the circle as I O B, be given in parts of the right angle T O F; and I G be equal to the arc T B F; find a right line N; which ſhall be a fourth proportional to the angles T O F, I O B and the right line I G; this line N is equal to the arc I T B; and a mean proportional betwixt I O, and $\frac{N}{2}$, is the ſide of a ſquare which is equal to the area I O B I; join I B, and from the laſt mentioned ſquare take the trinangle I B O; the remaining magnitude is equal to the circular ſegment I T B.

Cor. 3. The ſquare upon B F is equal to the difference of the areas of the circle and its circumſcribing ſquare.

Problem. If the circumference of a circle, whoſe diameter is unity be denoted by 3.1416; it is required to find a right line which ſhall approximate very nearly to $\frac{1}{4}$ of this number, or .7854.

Conſtruction. Draw I B, as in Theorem 1ſt, and make B G perpendicular to I F; then $IG \times 1 = IB^2 = .8000$; conſequently $IG = .8000$, which is greater than .7854. Let the reader take *g* in I G, ſo that I *g* may be of the required length; then as $8000 : 7854 :: IG : Ig$; but 8000 is to 7854 nearly as 55 to 54; therefore divide I G into 55 parts and I *g* will be 54 of theſe parts. Draw *g b* perpendicular to I G; join I *b*; and the ſquare upon I *b* will be nearer the truth than that upon I B. If a more complete approximation be required, it may be diſcovered by the method given in the ninth Problem of Emerſon's Arithmetic. Q. E. F.

It is the buſineſs of the practical geometrician to determine the value of theſe propoſitions in practical geometry. The ingenious Mr. Boſwell conſiders the firſt theorem to be of utility; for which reaſon I imagine any improvement in the diſcovery will prove acceptable not only to the inventor, but to ſeveral of your readers who are artiſts and mechanical geometricians.

JOHN GOUGH.

IX.

On the Culture, Properties, and comparative Strength of Hemp, and other Vegetable Fibres, the Growth of the Eaſt Indies. By Dr. WILLIAM ROXBURGH, *of Calcutta* *.

DEAR SIR,

YOUR letter of the 16th of May, 1799, I received on my return to Bengal in October laſt; but that from the Society eſtabliſhed in London, for the Encouragement of Arts, &c. of which you are a member, is not to be found.

Ill adviſed experiments for the culture of hemp in India.

I was rather ſurpriſed, on my return to Bengal, to find the directors had ſent out a perſon (Mr. Sinclair) to eſtabliſh the cultivation of hemp, a thing I had begun ſome time before. Even on the coaſt of Coromandel, ten or twelve years ago, I made a moſt ſucceſsful trial, the reſult of which was laid before that government, to be ſent to the Honourable Court of Directors; and again in Bengal, ſince my appointment to the ſtation I now hold. Mr. Sinclair is dead, and the experiment is ſtill carried on in a moſt expenſive manner; whereas it could be continued where it was firſt begun, in the botanic garden, at no expence, and with more proſpect of ſucceſs. Should government continue to be the cultivator, the price will be enormous. Eighty pounds weight is all, I believe, that is yet forthcoming, and coſts from 10,000 to 20,000 rupees. Such experiments throw a complete check in the way of all attempts to introduce new, or improve old, branches of agriculture and commerce. A ſmall premium ſhould be offered to the natives, and honorary rewards to Europeans, after the example is ſet on a ſmall and not expenſive ſcale.

Beſt method of promoting its cultivation.

Botanic garden at Calcutta.

The botanic garden was at firſt made very large, four times more than was neceſſary for ſuch a garden, the intention of which was merely to make experiments, and to invite the natives to ſee and profit by the examples in huſbandry carried on there.

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* Communicated to the Society of Arts, 1804.

Bow ſtring flax: the ſtrongeſt of vegetable fibres.

A quantity of my bow-ſtring flax was, I underſtand, ſent from the coaſt about two years ago, for the directors. I could wiſh to know what was done with it; for, to me, it ſeems to be the ſtrongeſt vegetable fibre we are acquainted with. I mean to ſend ſome by the January ſhips from hence, through the medium of this government, and wiſh it may fall into your hands, and that its qualities may be properly examined by the Society for the Encouragement of Arts. I am really ſorry that the letter, inviting me to become an honorary correſponding member of that Society, ſhould have been loſt. I beg you will aſſure the Society, that I am ſenſible of the honour they have done me, and ſhall be very happy to have it in my power to contribute my mite to promote the views of that laudable inſtitution.

When any new object, promiſing to become uſeful in the arts or manufactures of our country is diſcovered, and reported to your Society (for example, the bow-ſtring flax,) the Society will probably addreſs the Court of Directors, and recommend the cultivation and importation into England of the commodity itſelf.

Excellent qualities of the *arrow* palm; for its fibres; its wine; and its ſago.

Another object, of more national importance, which I recommended to this government, before I went to the Cape, was the growth of one of the moſt noble of the palms, the *arrow*, mentioned in Marſden's Hiſtory of Sumatra, page 77, and ſaid to yield at an early age (from five to ſeven years,) fibres ready prepared by nature, flexible, ſtrong, and moſt durable, and the moſt convenient for cables and cordage of all kinds, that can be deſired. It alſo yields great abundance of palm wine, which can be converted into ſugar or ardent ſpirits; and when the tree is old, its pith is the baſis of the ſago we ſo much value. I have diſtributed many hundred plants, and have ſtill a great number in the garden*, beſide many thouſand ſeeds in the ground. Drawings, and a deſcription of the moſt valuable tree, were ſent to the directors, under the name of *ſaguerus rumphii*; but as the trees from which they were taken have advanced in ſize and age, a new ſet of drawings, and a new deſcription, of the old one corrected, becomes neceſſary. They will be ſent to the Directors this ſeaſon. The object may be ſuch as your Society wiſh to attend to; and on that account I have ventured to trouble you with the above ſtatement.

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* Feb. 1801. About 100,000 plants have been reared in this botanic garden ſince the date of this letter.

I am, dear Sir,
Your moſt obedient Servant,
W. Roxburgh.

Calcutta, Dec. 24, 1799.
Robert Wiſſet, Eſq.

Dear Sir,

Experiments on hemp.

MY letter of the 24th of December, 1799, I am afraid, has not reached you. The experiments on hemp, therein mentioned, have not, I believe, thrown much additional light on the ſubject. My friend, Capt. Burrows, of the Earl Howe, has done more to make our own indigenous ſpecies (the *ſun* of the Bengaleſe) better known, than any other perſon I am acquainted with.

Comparative examination of vegetable fibres.

For theſe laſt twelve months my attention has been much taken up in collecting and comparing the various vegetable fibres of Aſia, &c. uſed for cordage, cloths, and paper. The reſult of theſe I have lately preſented to the ſupreme government, to be ſent to the Hon. the Court of Directors, in reply to the 79th paragraph of their general letter of the 7th of May laſt. This paper may be intereſting to your Society, particularly at this time, when the attention of all good patriots is drawn towards the diſcovery of a ſubſtitute for Ruſſian hemp. This paper, with my former eſſays, contain much information on the ſubjects therein mentioned.

Neceſſity of a ſubſtitute for Ruſſian hemp.

The *ſun* plant is moſt promiſing.

The *ſun* of the Hindoos, which is the prepared fibres of the bark of a well-known Indian plant, the *crotalaria juncea* of Linnæus, ſtill appears to me to be the moſt promiſing ſubſtitute for hemp which has come to our knowledge; I mean, when every circumſtance, relative to its quickneſs of growth, its being already univerſally known and cultivated by all the nations of India, its low price, pliableneſs, ſtrength, durability, &c. &c. are taken into conſideration. All that can be neceſſary for the procuring and tranſporting to England any quantity of this material, is to enſure the cultivator a certain price, and ready market for the commodity; and to have it properly cleaned and packed, to render the freight as low and convenient as poſſible. Cleaned ſamples of this very ſubſtance were ſent by me, ſix or ſeven years ago, to the Directors; ſo that the fault is not mine, if it is not already better known than it ſeems to be.

The diſcovery of a ſubſtitute for Ruſſian hemp is certainly an object of the firſt magnitude. If *ſun* is found to be the beſt ſubſtitute yet diſcovered, and coſts in India, ſay, when properly cleaned, ten pounds per ton, and the freight ſixteen pounds, there will ſtill be a conſiderable profit to the merchant, particularly in times of war; for, I believe, it rarely happens that hemp ſells ſo low in London as thirty pounds per ton. Should the ſubject appear of conſequence to you, I beg you will call the attention of the members of your Society to it.

It can be afforded at a reaſonable rate.

That I may not encroach too much on your time, I will cloſe this letter by referring you to my friend Mr. Boſwell, the late Marine Storekeeper and Naval Paymaſter, for any farther information you may want. I ſend this package by him. Mr. Babb, late a Member of the Board of Trade here, can alſo give you much information.

I am, dear Sir,
Your moſt obedient Servant,
W. Roxburgh.

Calcutta, Feb. 27, 1801.
Robert Willet Eſq.

Obſervations on the Culture, Properties, and comparative Strength of Hemp, Sun, Jute, and other Vegetable Fibres, the Growth of India, communicated by Dr. ROXBURGH.

HEMP, conſidered merely as an article of trade, is an object of the firſt importance to the merchant; but, when we reflect on its various uſes, and obſerve that hardly any art can be carried on without its aſſiſtance, or of ſome other ſubſtitute; the objects it embraces are immenſe, and there are few that better deſerve the attention of the philoſopher or intelligent artiſt. Cordage makes the very ſinews and muſcles of a ſhip, and every improvement which can be made in its preparation, either in reſpect to ſtrength, pliableneſs,

Importance of hemp and its very extenſive uſe in ſociety.

or durability, or in bringing to light ſubſtitutes equally good or better, particularly where hemp itſelf cannot well be had, muſt be of immenſe ſervice, particularly to the mariner, and to the commerce and the defence of nations.

Its cultivat on in our colonies intitled to ſupport and encouragement.

The cultivation of this important plant in our colonies has not only, at all times, met with encouragement from the government, but alſo of late from the Eaſt-India Company in Bengal, where extenſive experiments were begun by the late Mr. Sinclair, and after his death carried on by Thomas Douglas, on the culture of hemp and flax, on account of the Honourable Company. A clear and impartial ſtatement of theſe trials is to be wiſhed for, as it will, no doubt, throw much light on the cultivation of hemp in India, and enable us to proceed with greater proſpects of ſucceſs than ever.

It may be very beneficially produced in India.

My own experiments and inquiries on the ſame ſubject, both on the coaſt of Coromandel and Bengal, have been many, though not extenſive. Their reſult leads me to think, that hemp may be cultivated to great advantage over the interior parts of Bengal and Behar, where the ſeed ſhould be ſown about the beginning of the periodical rains, or earlier, if there have been frequent ſhowers, on elevated ſpots of rich loamy ſoil, ſuch as the Ryots cultivate tobacco, ſun, and paat on, near their habitations. In ſituations of this nature it thrives well, and will be eaſily attended to. At firſt, ſome encouragement will certainly be neceſſary, to induce the Ryots to undertake this new branch of agriculture. For, although the plant is perfectly familiar to every Hindoo, yet the cultivation on an extenſive ſcale, for the fibres of its bark, is perfectly unknown to them. I would therefore ſuggeſt, that they ſhould pay no rent for the ground ſo occupied for a certain period; that ſeed ſhould be given gratis; that they ſhould be enſured a certain price for the hemp; and finally, a reward or premium to the perſon or perſons who produced the greateſt quantity of the beſt hemp within a ſtated period.

The Hindoos cultivate it only for the ſeed.

Seaſon for growing it.

In many parts of Bengal, particularly where the land is ſo low as to remain humid through the dry-weather ſeaſon, hemp thrives luxuriantly during the cold ſeaſon; but the water is then too cold for macerating the plants to the greateſt advantage: one day in June, July, or Auguſt, has more effect in looſening the bark, than eight in December, January, or February; conſequently, the prolonged immerſion injures the quality.

quality of the hemp much. The rainy ſeaſon is therefore preferable for the cultivation and maceration, even if the plant grew better during the cold, which is by no means the caſe, particularly on lands elevated above the level of the annual inundation of the low rice-fields. We muſt therefore content ourſelves with one crop in the year; for it is a very falſe notion, and a very prevailing one, that the fertile fields of Aſia produce at leaſt two crops annually; as well might we ſay, that the fertile lands of England yield at leaſt two, becauſe a well-managed garden, near London, or ſome other large city, will produce repeated crops in the year: for in India, by great care and induſtry, a ſpot here and there will produce two or more crops. The burning heats of Aſia, while they laſt, are as unfavourable for vegetation, as the froſts of winter in Europe.

One annual crop only can be had.

Beſides hemp and flax, the vegetable kingdom, particularly that natural diviſion called by Linnæus, Columniferæ, abounds in plants which produce materials fit for cloths, paper, and cordage: almoſt every nation or country poſſeſſes ſomething of the kind peculiar to itſelf. To aſcertain what theſe are, as well as to find out new ones, to try their comparative ſtrength, durability, texture, &c. has, at various leiſure hours, employed my attention for many years paſt. Drawings and deſcriptions of many of them have been already laid before the honourable Court of Directors. There are, however, ſome others which remain to be brought under view and compared with the kinds we are beſt acquainted with, which I have attempted in the following experiments. Many other ſorts are alſo mentioned by various authors and travellers, of which I know nothing more than the names. Two of theſe are mentioned by Marſden, at pages 75 and 76 of his hiſtory of Sumatra. Others are peculiar to Pegu, &c. &c. Theſe I muſt omit for the preſent, and confine myſelf to ſuch as I am more intimately acquainted with. The better to enable me to proceed in this inquiry, I have cultivated, in the Botanic Garden at Calcutta, many of the plants themſelves which produce the materials hereafter mentioned; prepared their fibres in general by maceration, &c. as with hemp and flax in Europe. And, to compare their ſtrength (plain, tanned, and tarred,) had them made into cords, compoſed of three ſimple yarns, as nearly of the ſame ſize and hardneſs as a

Other vegetables particularly the columniferæ, fit for the uſes of hemp.

Deſcriptive outline of the author's experiments on various plants for cordage, &c.

Preparation of the fibres.

Hindoo

Cordage manufactured, and tried in the white, and alſo tanned and tarred.

Hindoo rope-maker could make them; but, in ſpite of my utmoſt care, they were always too hard twiſted to be of the greateſt poſſible ſtrength *. Of each ſort there were ſix, when there was a ſufficient quantity of the fibres. Three of them were about the ſize of a log-line, and three a ſize larger than a whip-cord; one of each ſize and ſort was kept white: the average number of pounds which broke them, (for repeated trials were made, and always with lengths of exactly four feet), will be found in the firſt and ſecond columns on the right. One of each was tanned † with the aſtringent fruit called *gaub* by the Bengaleſe, (Embryopteres Glutinifera, Roxb. Coromandel Plants, Vol. I. No. 70.) Their ſtrength is repreſented in the third and fourth columns of the following table. And, laſtly, one of each ſort was tarred: their reſpective ſtrengths will be found in the fifth and ſixth columns.

Tanning ſtrengthens animal fibres, but quere vegetable?

We know the tanning principle ſtrengthens the fibres of leather, (animal fibres) but are not ſo clear that its operation on vegetable matter is uniformly the ſame. The attention beſtowed to aſcertain this point in theſe experiments will, at leaſt throw ſome light on the ſubject; and may induce others, better qualified, to extend the inquiry (here in India), where tanning materials abound.

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* The experiments of Reaumur, Sir Charles Knowles, and Du Hamel, uniformly prove, that when hemp-rope is twiſted to the uſual hardneſs, which is that which brings them to two-thirds of the length of their reſpective yarns, their ſtrength is leſſened by nearly one-fourth, when compared with ropes conſiſting of the ſame number of the ſame yarns twiſted up to only three-fourths of their length. There will be no difficulty in accounting for this difference, if we conſider that a ſkein of fibres may be twiſted ſo very hard, as to break with any attempt to twiſt it harder. In this ſtate the fibres are already ſtrained to the utmoſt, and cannot ſupport any weight or additional ſtrain.

† The idea of tanning cordage is far from being new; for the fiſhermen of Aſia, as well as of Europe, not only tan their nets and lines, but alſo their ſails, to give them additional ſtrength and durability. The ſame proceſs might be productive of the ſame effects, if employed on cordage made of the materials (No. 2, 5, 6, 7, 8, and 15), ſpecified in theſe experiments, which induced me to recommend its being tried with ſun cordage, in my letter to the Board of Trade, in Auguſt, 1797.

Whether tan will preſerve wet rope?

Tar does not ſtrengthen cordage; but the contrary.

Tar not produced in tropica Aſia.

Another point of the utmoſt importance to be aſcertained is whether tanned ropes will be preſerved by the tan, with which they are impregnated, when ſtowed away wet; as tarred ropes are preſerved by the tar when ſo circumſtanced. It is nevertheleſs a well-known fact, that tarred cordage, when new, is weaker than white; and that the difference increaſes by keeping. Tar can therefore only be employed to preſerve cordage, and not to ſtrengthen it; ſo that if tan will add ſtrength, or even not weaken vegetable fibres, and at the ſame time preſerve them; of how great advantage to the nation would the diſcovery of a convenient practicable proceſs be. For all cordage, expoſed to be alternately very wet or dry, requires to be impregnated with a preſervative. And, to conclude this long note, I beg leave to obſerve, that tar is not the produce of the warmer parts of Aſia. Are we, therefore, to conclude, that no material, the produce of theſe parts, can be applied for the ſame end? Let us not entertain any ſuch idea. Nature is abundantly kind, and furniſhes every country and climate with what is moſt proper for the uſe of its inhabitants.

Statement of experiments on the cordage.

The annexed ſtatement of the experiments made on the ſubſtances there ſpecified, can only be deemed an attempt towards aſcertaining their relative ſtrength; and though they are the average reſult of ſeveral trials made on the ſtrength of each cord, plain, tanned, and tarred, yet I muſt acknowledge they cannot be deemed any thing more than a firſt eſſay, chiefly owing to the lines being in general ill laid, ſome more and ſome leſs twiſted, and by no means to be compared with thoſe of Europe. For in ſome inſtances, I found a ſmall one ſuſtain a greater weight than a much larger, made exactly of the ſame materials. It is therefore my intention to repeat them on a larger ſcale, and, if poſſible, with better made lines; for every thing depends on their being exactly of the ſame ſize and degree of twiſt.

COMPARATIVE STATEMENT OF THE EXPERIMENTS.

NAMES of the PLANTS, and brief REMARKS on the various MATERIALS employed in theſe EXPERIMENTS.	Average weight at which each ſort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
No. 1. Engliſh Hemp: a piece of new tiller-rope, opened out and made into cords, like the reſt,	105	65	..	..	..	..
2. Hemp (Cannabis), the growth of this ſeaſon, from the Company's Hemp Farm, near Calcutta,	74	50	139	60	45	46
3. Coir: the fibres of the huſk of the Cocoa-nut; much uſed for cables and cordage over Aſia,	87	60	..	..	..	.
4. Ejoo (Saguerus Rumphii): the black horſe-hair-like fibres, which grow round the trunk of this ſpecies of Sago Palm,	96	79	..	..	..	..
5. Robinia Cannabina, Danſha of the Bengaleſe: the fibres of its bark, prepared by maceration from the plants that had nearly ripened their ſeed; they are then of a duſky grey colour, and harſh nature,	88	64	101	55	84	39
6. The fibres of the bark of No. 5, prepared by maceration, from plants coming into bloſſom; at which time they are beautifully white, ſoft, and gloſſy,	46	20	61	35	48	30
*7. Crotolaria Juncea, Sun of the Bengaleſe: the fibres of its bark, and univerſally known over India,	58	47	69	55	60	37
8. Corchorus Olitorius, Bunghi-paat of the Bengaleſe: the fibres of its bark, called Jute by the ſame people,	68	47	69	59	61	36
9. Corchorus Capſularis, Ghee-nalta-paat of the Bengaleſe: the fibres of its bark they call Nalta Jute,	67	47	..	..	..	..
10. Flax (Linum Uſitatiſſimum): the growth of the Company's Hemp Farm, near Calcutta	39	37	..	..	..	..

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* A cord, a very little thicker than a log-line, made of ſun ſail twine, broke with 148 pounds when dry; but, on being ſoaked in cold water for 24 hours, it bore, while wet, 222 pounds. This difference requires to be farther inquired into.

COMPARATIVE STATEMENT OF THE EXPERIMENTS—*continue*

Names of the Plants, and brief Remarks on the various Materials employed in theſe Experiments.	Average weight at which each ſort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small	Large.	Small.	Large.	Small.
11. Agava Americana: the fibres of its leaves. They are of a coarſe harſh nature, and white, - - - - - - - - - - - - - - -	110	71	79	78	78	38
12. Aletres Nervoſus—In Sanſcrit, Murva; Murga of the Bengaleſe: the fibres of its leaves made into theſe cords, after having been kept above one year, -	120	52	73	42	48	43
13. Theobroma Auguſta, Linn.—Abroma Auguſta, Hort. Kew.—Abroma Wheelerii, Kæn.—Woollet-comal of the Bengaleſe: the fibres of its bark prepared by maceration, &c. like hemp, - . - - - - - - - -	74	61	58	51	44	29
14. Theobrama Guazuma, Baſtard Cedar: the fibres of the bark of ſome ſtraight, luxuriant, young plants, - - - - - - - - - -	52	48	..	47	45	30
15. Hibiſcus Tiliaceus, Bola of the Bengaleſe: the fibres of its bark, and employed for cordage by the inhabitants of the South-Sea Iſlands, &c. - - -	41	..	62	39	61	..
16. Hibiſcus Manihot, a tall white-flowered variety: the fibres of its bark, which are beautifully white, gloſſy, and ſoft, - - - - - - - -	61	..	..	..	..	..
17. Hibiſcus Mutabilis: the fibres of its bark, and they are of a harſh nature and ill coloured, - - - - - - - - - - - - - - -	..	45	53	46	..	..
18. Hibiſcus, a new ſpecies, from the Cape of Good Hope, ſaid to be a native of Caffraria, where the fibres of its bark are ſpun, - - - - - -	..	22	..	..	..	..
19. Bauhinia, a large ſcandent ſpecies: the fibres of its bark, cleaned without maceration, and uſed to make ropes, &c. of by the people of Napaul, where the plant is common, - - - - - - - - - - - - -	69	39	..	..	..	..
20. The ſame as No. 19, only maceration was uſed to help to take the bark off the twigs with more care, - - - - - - - - - - - -	56	41	..	..	..	..
21. Sterculia Villoſa: the fibres of its bark. Cords are made of them by the natives of the eaſtern frontier of Bengal, to bind wild Elephants, when firſt taken,	53	..	..	..	..	..

The cords, when the trials were made, had been kept about ſix months after they were prepared, chiefly with the view of allowing the effects of the tan and tar to take place. The reſult of theſe experiments ſhow, that tan has in general added ſtrength, while tar has had a contrary effect; and in no inſtance is this more clearly evinced, that in the common hemp (Cannabis) cultivated in Bengal.

Ejoo and Coir.

To Ejoo and Coir, neither tan nor tar ſeem applicable; and in ſeveral of the other experiments, I had not a ſufficient quantity of the materials to make the neceſſary number of cords, viz. ſix of each ſort, to try with tan and tar, as well as in their natural ſtate. At ſome future period, I hope to be more fortunate in procuring larger ſupplies of the materials, and alſo to add ſome other other ſorts, ſuch as the—

Bow-ſtring fibre.

Rajemahl bow-ſtring fibre, the produce of a new ſpecies of Aſclepias, diſcovered by William Roxburgh, junior, amongſt the Rajemahl Hills;

New Zealand hemp.

New Zealand Hemp;

Hibiſcus Cannabinus, and ſome other of the ſame natural order (Columniferæ); (for in general their barks abound with ſtrong fibres; witneſs the foregoing table, where ſix of them are to be found);

The leaves of a new ſpecies of Andropogon, &c. &c.

APPENDIX,

Containing Remarks on ſome of the Plants mentioned in the foregoing Table.

Remarks on hemp.

No. 2. Hemp, or Cannabis Sativa.—Banga, in Sanſcrit, Bunga, Bungh, or Bung, of the Hindoos; Bang, of the Perſians; Kinnub, of the Arabians, is no doubt our own famous plant, now ſo common and uſeful in Europe. I have at different times examined various figures and deſcriptions, as well as the plants reared from Europe ſeed, comparing them with our Indian plant through its various ſtages, and can diſcover no difference whatever, not even to found a variety on. Perhaps few vegetables, ſo widely diffuſed over almoſt every part of the known world, and under the immediate management of man, have undergone leſs change. It is perfectly familiar to all the nations of India, I may ſay of all the warmer parts

It is well known in India, but not for its fibres;

parts of Aſia; yet I cannot diſcover that the fibres of the bark have ever been employed for any purpoſe. It is cultivated in ſmall quantities every where, on account of its narcotic qualities.—The leaves of the male plant, and flowers of the female, are the parts in moſt general uſe.

nor in China, &c.

I have repeatedly applied for the ſeeds of all plants reared in China, and other countries to the Eaſtward of the Bay of Bengal, as well as to almoſt every other part of India we have any communication with, for an account of the plants employed to ſupply materials for clothes and cordage, and for their ſeeds; but could never learn that Cannabis was one of them; nor were its ſeeds ever ſent to me as ſuch.

Ejoo; very ſtrong, but no elaſtic.

No. 4. The great ſtrength of this ſubſtance makes it a very deſirable object. For a deſcription and drawing of the tree I refer to thoſe* which accompanied my letter to the Moſt Noble the Governor-General in Council, under date the 2d of January, 1800. The fibres employed in theſe experiments were taken from trees growing in the Botanic Garden at Calcutta, where they thrive well. I could obſerve, during the trials made in breaking the cords of this ſubſtance, that they were not ſo elaſtic as thoſe of Coir, which will probably render it leſs fit for cables, but better for many other uſes. Coir is certainly the very beſt material yet known for cables, on account of its great *elaſticity* and ſtrength.

Other plants:

Nos. 5, 6, 7, 8, and 9. Theſe four plants have already been figured and deſcribed by me, in a memoir ſent through the Governor-General in Council, to the Hon. the Court of Directors, in December, 1795. Since writing that paper, I have learned, that *ſun* (Crotolaria Juncea) is almoſt univerſally employed, over the warmer parts of Aſia, for cordage. On the Malabar coaſt, I find it is generally named by the gentlemen at Bombay after the province where reared. It is uſed in that place for lacing their cotton bales, on account of its great ſtrength. Samples of three ſorts, viz. Malwan, Rajapore, and Salſette, were ſent to me, from them, by Dr. William Hunter; and am induced to think, little or no maceration is

The *ſun* or crotolaria juncea.

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* A former ſet, No. 1173, ſent in to Government on the 23d of November, 1797, were not ſo correct as could be wiſhed, on account of their having been taken from young trees, juſt coming into bloſſom the firſt time.

employed in taking the bark from the ſtalks, or in cleaning the fibres, which may add to its ſtrength: for certainly maceration, particularly if long continued, muſt weaken freſh vegeta fibres conſiderably. The ſame gentleman ſent me ſeeds of the Salſette ſort: they have produced plants now in bloſſom, and from them have aſcertained the identity of this ſpecies.

In ſome parts of Bengal, a moſt luxuriant variety is cultivated immediately after the rains, which often grows to the height of twelve or fourteen feet; while the common ſort is generally reared in Bengal during the early part of the wet ſeaſon, and grows to only about half the height of the former.

Manſta of the Bengaleſe;

I muſt farther obſerve, that the fibres of No. 5, poſſeſs great ſtrength, and it ſeems to me to be one of the moſt fit of any of our Indian productions for cables and cordage. The plant grows generally to the height of from ſix to ten feet, the fibres long, but harſher than thoſe of hemp, if not cut at an early period. It is very generally cultivated about Calcutta during the rains. An acre yields of the half-cleaned ſubſtance (the ſtate in which the natives carry it to market), about 600 lbs. weight, and ſells for about a rupee and a quarter per maund of 80 lbs.

beautiful but weak fibres.

No. 6. By cutting No. 5, the laſt-mentioned plant, when beginning to bloſſom, we have the moſt beautiful ſhining white fibres that can well be conceived, but (by my experiments) greatly weaker that when the ſeed is ſuffered to be nearly ripe before the plants are cut.

Flax cultivated in India for its ſeeds only.

No. 10. Flax, the plant, is very generally cultivated during the cold ſeaſon, over the interior parts of Bengal and Behar, merely for the ſeeds, from which oil is obtained. The flax itſelf the Hindoos ſet no value on; for, after they have gathered the ſeed, they throw away the ſtalks as uſeleſs, having no knowledge of the fibres which their bark yields. Samples of the flax have repeatedly been procured by the Board of Trade, and ſent to England to the Hon. Court of Directors; ſo that it is from England we may expect to learn its properties. If the flax has been found good, large quantities may be reared at a ſmall expenſe; as the ſeed alone, which the crop yields, muſt be more than equal to the charges, to render it profitable to the farmer.

Agava Americana; of no great promiſe.

No. 11. This Agava is of ſlow growth; on that account I doubt if ever it can be advantageouſly cultivated; but, where found wild in plenty, it may be manufactured at a trifling expenſe. Its great ſtrength renders it an object worthy of attention. The fibres are coarſe, conſequently rope made of them harſh to the feel.

Aletris nervoſus.

No. 12. Drawings, and a deſcription of the plant Aletris Nervoſus Roxb. the method of extracting the fibres, with a quantity of the ſubſtance itſelf, were ſent to the Hon. the Court of Directors, through the Madras Government, above ten years paſt. I alſo gave a large quantity to Mr. Bebb, when he left Bengal in January, 1800, to take to England with him for trial there. The plant grows fully as well, and is as common as on the Coromandel coaſt. There has lately been about a biggah (third of an acre) planted out with it in the Botanic garden, the better to determine the expenſe, and the annual produce of any given quantity of ground.

Abroma auguſta, very promiſing.

No. 13. This plant, a native of various parts of India, New South Wales, Philippine Iſlands, &c. has been long known to botaniſts; yet I cannot, with all the attention that I have been able to beſtow on the ſubject, find that the fibres, ſo abundantly interwoven through its bark, have ever been uſed or even taken notice of by any other perſon; ſo that I think we may look upon it as a new diſcovery, deſerving of more than common attention, on account of the beauty, fineneſs, and ſtrength of theſe fibres.

Its growth.

It is perennial, grows luxuriantly in the Botanic garden, and has been cut down twice within theſe ſix or ſeven months; ſo that I think it will, at leaſt, annually afford two or three crops of ſhoots fit for yielding this ſubſtance. My experience does not yet enable me to ſtate how much may be the yearly produce of an acre, but can venture to prognoſticate as large a produce as can be obtained from an acre of Dancha, Jute, Sun, Hemp or Flax.

Treatment to obtain the fibre.

To render this bark ſeparable from the half-ligneous ſhoots it covers, to ſoften its external lamina, or epidermes, and the parenchymatous ſubſtance which firmly connects the fibres in their natural ſtate, maceration in ſtagnant water for from four to eight days, during the warmer parts of the year, anſwers well whilſt three times as many days are ſcarce ſufficient during the cold ſeaſon; indeed, the proceſs is ſcarcely practicable then; beſides, the fibres are greatly weakened by the length of the maceration.

Immediately on being taken out of the water, and while wet, the ſhoots are ſingly taken in the hand, rubbed with ſome coarſe materials, ſuch as a little dry graſs or gunny, [illegible] remove the exterior pulpy lamina or epidermis of the bark, which is deſtitute of fibres. This part of the proceſs is eaſily effected; and when done, the clean ſhoots are to be made up into ſmall bundles, and placed under weights, or ſome other equal preſſure, to keep them firm at the middle and top, either under the ſurface of the waters, or out of it; the fibrous bark is then ſeparated with the fingers from a ſmall portion at the end of the ligneous ſhoot or ſtalk, which the operator takes hold of, and draws out one by one; when theſe are removed, the pulp, or parenchyma, which fills the interſtices between the fibres, and connects them together, forming in the living plant that part of the bark which may be called its inner lamina, or cellular tiſſue, is immediately waſhed out in cold water, and the clean fibres ſpread out in the ſun to dry. Such was the ſimple proceſs by which this ſubſtance (which may very properly be called Indian Hemp or Flax) was prepared.

I have now under cultivation about the third of an acre (a biggah) of ground in the Botanic garden with this plant; the reſult ſhall be carefully noted from time to time. It might have been prudent to have withheld this account until that time; but the ſtrong deſire of making known a diſcovery, which may in a ſhort time become beneficial to the public, induces me to be thus precipitate.

For a farther account of this plant, I beg leave to refer to my drawing and deſcription thereof, ſent to the Hon. the Court of Directors ſome years ago, and numbered 115.

W. ROXBURGH.

Botanic Garden, near Calcutta,
Jan. 31, 1801.

Additional Experiments on the Strength of Sun, (No. VII.)

Additional experiments on the ſtrength of *ſun.*

Some tanned ſail-twine, made of this ſubſtance four years ago, for the inſpection of the Marine Board, was made into a cord of three ſtrands; each of the ſtrands compoſed of four threads of the ſail-twine.

Some more of the ſame ſail-twine, tanned twelve months ago, was made into a ſimilar cord, and another was made of the white unprepared twine.

The firſt, which had been tanned four years, broke with 110 lbs. when dry, and with 130 lbs. after having been ſteeped in water 24 hours.

The ſecond, which had been tanned one year, broke with 123 lbs. when dry, and with 140 lbs. after ſteeping 24 hours.

The third, or plain white, broke with 148 lbs. when dry, and with 222 lbs. after ſteeping 24 hours.

X.

Deſcription of the Bavarian Method of evaporating Saline Waters. By M. BONNARD.

Bavarian method of evaporating ſalt waters.

THIS new method, practiſed in Bavaria, has been introduced into the ſalt work of Moyenvic, by M. Cleiſs, inſpector of the ſalt-works of Bavaria.

The pans are compoſed of ſquare plates of caſt iron, of 4 millimetres in thickneſs, and 4.76 centimetres on each ſide. Theſe plates are joined by their edges, which are turned downwards, and conſequently without the pan: they are ſolidly united by a piece in the form a ſquare gutter which receives the edges, and is ſecured by a great number of ſcrews.

An evaporating houſe is compoſed of ſix pans, of this conſtruction, diſpoſed in two rows; but theſe pans have different uſes, which require a particular arrangement.

That in the middle of the back row is the ſmalleſt; it has no particular fire-place, but it is heated by the junction of the chimnies from the other fire-places. The ſalt-water depoſits its impurities in it; it is called the ſmall pan.

From the ſmall pan the ſalt water paſſes into the graduating pan, which is lower than the firſt, and placed in the middle of the front row; it is there kept in a ſtate of conſtant ebullition: the water is concentrated in it to 20 degrees, and depoſits a part of its ſulphated lime.

From the graduating pan the ſalt water paſſes into the preparing pans, which are lower than it, and ſituated at the two extremities of the back row: there it is alſo kept conſtantly

boiling,

Bavarian method of evaporating ſalt waters.

boiling, it is completely concentrated, and depoſits all its ſulphate of lime; it is then paſſed into the cryſtallizing pans, ſtill lower than thoſe of preparation, and placed at the two extremities of the front row: there the water ſcarcely boils, and the ſalt cryſtallizes.

Each pan, with the exception of the ſmall pan, has a particular fire-place, the chimnies of which paſs round the ſides of the pan: they unite under the ſmall pan, by which means there is little heat loſt.

Theſe pans are placed two and two in chambers of wood, the joinings of which are well ſecured, which cloſe them hermetically: theſe chambers are low, and their ceilings are perforated in the middle with holes terminating in a tube, by means of which the aqueous vapour is diſengaged with rapidity. The chambers for the preparing and cryſtallizing pans have their ceiling pyramidal, or in the form of a reverſe hopper, while that for the ſmall pan and the graduating pan is horizontal.

The ſaline waters are paſſed ſucceſſively into theſe four kinds of pans; the workmen penetrate into the chambers, in the midſt of the vapour, to open the communications. This operation is performed every ſix hours, and the water in each pan is reſtored to the level at which it ſtood ſix hours before. Every three hours the ſalt in the cryſtallizing pans is collected, it is brought with ſcoops to elevations on the front edge of the cryſtallizing pans, where it drains; it is afterwards carried into drying rooms, which ſurround the outſide of the chambers: theſe are ſpaces covered with iron plates; they are warmed by heat-tubes leading from the fire-places.

Every eight days they take away the ſulphate of lime, throw out the mother-waters, and break the ſhell, that is to ſay, the incruſtations of ſalt which adhere to the bottoms of the pans: every twenty-four days the work is entirely ſtopped to repair the pans; an operation which is performed by the workmen themſelves.

Economy of fuel.

It has been found that this method of evaporation affords a ſaving of more than one-third of the fuel.

An improvement has lately been made in this proceſs at Dieuſe: the ſmall pan has been ſuppreſſed, and the drying rooms have been replaced by auxiliary pans, in which a coarſe ſalt is made.

The heated drying rooms are uſeleſs when the humidity of the ſalt ariſes from the muriate of lime it contains.

Bavarian method of evaporating ſalt waters.

Explanation of the Plates III. and IV.

Fig. 1. Plan of the pans.

No. 1. Small pan.
No. 2. Graduating pan.
No. 3. Preparing pan.
No. 4. Cryſtallizing pan.

The diſpoſition of the plates of iron which compoſe theſe pans, is ſhewn in No. 2.

a, a, Elevation on which the ſalt is placed to drain, as it is taken from the cryſtallizing pans.

b, b, b, Wooden partitions which ſeparate the chambers.

c, c, c, A raiſed wooden ledge which ſurrounds the pans.

Fig. 2. Section of the evaporating chamber which contains the pans 1 and 2.

d, d, d. Heat tubes which give heat to the ſmall pan, and contribute to heat the others.

e, e, e, Fire place for the pans.

i, i, i. Pillars of caſt iron under the gratings *g, g, g,* which ſupport the bottom of the pans.

h. Wooden chamber which contains the two pans.

k. Opening by which the vapours eſcape.

Fig. 3. Section of the evaporating chamber which contains the pans 3 and 4.

a. Elevation on which the ſalt from the cryſtallizing pans is placed to drain.

The other letters indicate the ſame parts as in the preceding figures.

Fig 4. Method in which the plates of iron are joined to form the pans.

a. The iron plate.

b. The iron gutter which receives the edges of the plates, and is ſtrongly faſtened with ſcrews.

i, i. Pillars of caſt iron which ſupport the bottom of the pan.

XI.

Letter of Inquiry reſpecting the late Dr. Irvine's *fundamental Experiment on the relative Capacities for Heat of Ice and Water. With an Anſwer by Mr.* Irvine.

Requeſt that Mr. Irvine would deſcribe his father's experiment on the capacities of ice and water.

A CORRESPONDENT would be much indebted to Mr. Nicholſon if he would convey by publication in his Journal, or otherwiſe, the following requeſt to Mr. Irvine. That gentleman in his paper in the Journal of laſt month* mentions his having in his poſſeſſion the experiments of Dr. Irvine on the capacities of bodies. It has always been a wiſh with theſe chemiſts who have attended particularly to this ſubject to know in what manner Dr. Irvine made the experiment to aſcertain the comparative capacities of ice and water. If either of the bodies during the experiment change its form, if the ice were melted, or the water congealed, it becomes leſs deciſive, as it may be objected by thoſe who maintain the opinion that latent caloric exiſts in bodies either in whole or in part in a ſtate of chemical combination, that the reſult might ariſe from ſuch a combination, and not from a change of capacity. But if the experiment were made in ſuch a manner that no change of form took place, which though difficult is poſſible, Dr. Irvine's theory, which is ſo much ſuperior to the other, is unequivocally eſtabliſhed. It would be conferring a favour on the chemical world, if Mr. Irvine would take the trouble of ſaying in what manner the experiment was conducted, providing ſuch a notice would not interfere with the intention he has announced of giving a more full account of his father's inveſtigations, an account which would be eagerly received by chemiſts.

Change of form would influence the reſult.

September 24th, 1803.

Reply by Mr. Irvine.

The experiments may be made without being liable to the objections before ſtated;

WITH regard to the enquiry of Mr. Nicholſon's correſpondent, it would give me pleaſure to inform him of any circumſtance within the ſphere of my own knowledge that ſhould tend to add to the illuſtration or proof of my father's theory.

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* Sept. 1803. See our Addreſs to Correſpondents of laſt month.

There does not ſeem to me any difficulty in explaining ſatisfactorily how experiments on the capacities of ice and water may be conducted without being expoſed to the objections above ſtated; at the ſame time, though theſe experiments may be concluſive as far as they go, I have not been accuſtomed to conſider them as altogether ſo deciſive of all arguments upon this ſubject.

—but may not be unexceptionable.

Account of Dr. Irvine's method of experimenting.

The capacities of mercury, ſand, pounded glaſs and iron filings were taken with reſpect to water.

One of theſe was then examined againſt pounded ice at a temperature of the place always under 32°.

Example.

In a general way then the experiments of Dr. Irvine were conducted in the following manner. The capacity of water being taken at unity, pains were uſed to aſcertain the capacities of mercury, river ſand, pounded glaſs, and iron filings, with reſpect to water, and conſequently to each other in the ordinary manner. It is at preſent of no importance what proportional quantities of the materials were employed. This being done, the capacity of one of theſe ſubſtances was experimentally compared with that of pounded ice or ſnow.—Every precaution was uſed to enſure ſucceſs. The weight and capacity of the veſſel was determined, and the colder and hotter body alternately added to the other. The temperature of the air was always below 32, as was that of all the materials and of the veſſel. No water could therefore be formed. In his firſt experiments I believe Dr. Irvine uſed mercury, but afterwards I know that he preferred iron filings and ſand. For example, if the veſſel and room were at 11°, let half an ounce of powdered ice from diſtilled water, at temperature 30° be poured on four ounces of iron filings at 11°, let the temperature of the mixture be noted after ſtirring, the due allowances made for the heat gained by the veſſel and the air, and a proper calculation made. Let this experiment be reverſed by cooling the ice to 11°, and pouring the iron filings at 30° upon it, and let a calculation from this be compared with the former and corrected by it. Finally, let theſe experiments be compared with others where different quantities of materials are uſed, and of different temperatures, and you have a view of the method employed for determining this point by Dr. Irvine, which does not appear to be objectionable upon other grounds than all experiments for aſcertaining capacities are, none of which have any pretenſions to perfect accuracy. Dr. Irvine was far from being ſatisfied that his experiments were mathematically preciſe. But he uniformly found the capacity of ice to be leſs than that of water, and that in a greater ratio than is generally allowed.

The common notion, of ice and water being directly applied to each other, unfounded and impracticable.

With reſpect to the common notion that experiments on the capacity of ice are made by mixing it with water, it is altogether falſe in the caſe of Dr. Irvine. No doubt this may be done theoretically. It is eaſy to ſay that ice loſes ſo many degrees which heat water only ſo many. But then no ice muſt be melted, which if not impoſſible is extremely difficult, or what is equally ſo, the quantity melted muſt be found, and an allowance made for the latent heat, which is itſelf not preciſely aſcertained, at leaſt not with ſufficient accuracy for this purpoſe. I have only to add that this gentleman's enquiries would have been earlier noticed if I had been informed of them, which I was only a few days ago.

W. IRVINE.

April, 1805.

XII.

Short Account of ſome of the moſt remarkable Facts and Obſervations in an Aeroſtatic Voyage, made from Peterſburgh, by Meſſrs. ROBERTSON *and* SACHAROFF, *under the Sanction of the Imperial Academy.* W. N.

Aeroſtatic voyage from Peterſburgh.

THE notice of an aeroſtatic voyage performed by Meſſrs. Robertſon and Sacharoff from Peterſburgh, under the direction of the Imperial Academy of Sciences, June 30, 1804, of which an abridged account was given to the National Inſtitute of France*, is very intereſting, from the ſcientific views and conduct of the managers Lowitz and Robertſon, as well as for its other particulars.

Aſcent of the balloon with two obſervers.

Their balloon was a ſphere of 30 feet diameter, and roſe at a quarter paſt ſeven in the evening, with the aſcenſional force of one pound, the whole weight of ſolid matter (including 110 pounds of ſand for ballaſt) being 622 pounds. When they were over the river Neva, at the elevation of 108 toiſes or 620 feet, they deſcended a little by the condenſation of the gas; but roſe again by throwing out a little of the ballaſt. The uſual phenomenon of a ſlow rotation of the balloon preſented itſelf, which doubtleſs aroſe from the unequal action of the air againſt an irregular ſurface, as we ſee in moſt other bodies riſing or falling in a fluid.

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* Inſerted in the Annales de Chimie, LII. 121.

But

Their directio
obſerved by a
floating log;

—of paper, &

But one of the moſt ſtriking circumſtances attending this voyage is the rational means which theſe philoſophers made uſe of to determine, and in a certain degree to regulate their courſe. They made uſe of two inſtruments, a log and a teleſcope. The log conſiſted of two ſheets of very thin paper, blacked, and fixed at right angles to each other by a very light croſs of wood. This was ſuſpended from their car by a ſtring of ſixty feet in length, and affording a different reſiſtance to the air from that of the balloon itſelf,† it was found to draw the ſtring out of the perpendicular direction, or as the narrators ſay, *to follow the balloon*; ſo that by its poſition determined by compaſs they could aſcertain what direction they were purſuing. It alſo ſhewed by its relative riſe and fall whether the apparatus was deſcending or aſcending, before their barometer had indicated the ſlighteſt change.

Perpendicular
teleſcope by
which their
courſe and vel
city were ſeen
on the ground

Their teleſcope was applied to ſhew the direction of their courſe, and muſt have been much leſs ſubject to doubt than their log. Its application would be univerſal and perfect, if the earth could be ſeen at all times from the elevated regions of the air. It was directed perpendicularly downwards by means of a plumb-line, and having a conſiderable magnifying power, the objects upon the ſurface of the earth were ſeen moving acroſs its field of view, and their direction would moſt clearly aſcertain that of the car itſelf, and alſo its velocity. If, for example, the magnifying power were fifty times and the field of view one degree, the viſible ſpace included in that field from an elevation of two miles would be about 180 feet in diameter, in which objects of ſix or ſeven inches broad might be very well diſtinguiſhed through a favourable atmoſphere; and at ſo low a velocity as one mile an hour the whole field of view would be paſſed over in about twelve ſeconds. Hence we ſee that the method affords a conſiderable degree of accuracy, and will not in general require any great power of magnifying or delicacy of obſervation. The computation would be founded on the following problem, which will not preſent any difficulty to thoſe who are acquainted with theſe ſubjects, if the phyſical allowances for temperature in barometrical admeaſurements be admitted to be correct enough for this purpoſe.

Inveſtigation o
the degree of
accuracy of this
method.

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† Or rather, perhaps, becauſe not exactly in the ſame current of the atmoſphere. N.

Given

Problem for computing the velocity of a balloon by obſ. through a teleſcope.

Given the temperature on the earth and in the car, the height of the barometrical column, and the time employed in the apparent tranſit of an object on the earth through a given angle or field of view; to find the velocity of the obſerver.

Tabulated reſults for practice.

For practice it would perhaps be ſufficiently exact and convenient to compute a ſmall table, in which, neglecting the temperature, the velocity in miles per hour might be had by inſpection, when the height of the mercury, the time of tranſit, and the magnifying power were known.

The obſerved courſe, &c.

The aeronauts having noticed by their inſtruments what were the direction of the currents of air at different heights, found themſelves in one which carried them directly towards the Baltic. They therefore deſcended till they ſaw by the barometer that they had returned to a current which carried them inland; and afterwards again roſe much higher, and ſaw with great preciſion by their teleſcope the inſtant of their quitting the gulph.

A pigeon let go.

When the barometer ſtood at 24 inches they let go a pigeon, who flew with difficulty and would not quit the balloon; but upon being precipitated he in vain endeavoured to regain it, and at length deſcended rapidly towards the earth.

The dip of the magnetic needle is leſs at great heights in the air.

At ten at night the balloon had riſen to an height indicated by 22 inches of the mercurial column, the thermometer ſtanding at 4½ degrees (I ſuppoſe centigrade). Here it was that M. Sacharoff carefully obſerved a phenomenon which had been before remarked by M. Robertſon in his aſcent from Hamburgh, but at a much greater elevation. Their dipping needle was deranged; but on inſpecting the common compaſs, its needle was found to be no longer horizontal, the north end being elevated near 10 degrees.

This may afford means of ſhewing the heights, &c.

On this phenomenon they remark that the magnetic attraction probably diminiſhing as the ſquare of the diſtance may afford additional means of directing future obſervers in the atmoſphere, and even determine the elevations independently of the barometer. From the preſent elevation a pigeon being thrown down, fell ſo directly, that it was doubted whether he could have reached the earth alive.

Singular echo of the voice heard from the earth at the diſtance of two miles.

Darkneſs coming on, it became neceſſary to deſcend, during which the obſerver repeatedly made an experiment which alſo promiſes to be of great utility to voyagers in the air, as well as to enlighten our concluſions reſpecting the phenomena of ſound. When they ſpoke through a trumpet directed towards the earth, the voice was returned with extreme preciſion and without

No echo but when the trumpet was directed to the earth.

without ſeeming to have loſt any part of its intenſity. No repetition was made except when the trumpet was directed to the earth; and the intervals of reflection were different according to the elevation of the obſervers. The percuſſion impreſſed on the air by the ſound every time produced a ſlight undulation in the aeroſtat; whence they deduce an inference in favour of the ſuppoſed efficacy of cannon in partly modifying or averting the diſcharge of ſtormy clouds. In one of their experiments, the ſound employed ten ſeconds in its return, which would give a diſtance of about two miles out and home, if the ſame law of the velocity of ſound were ſuppoſed to prevail in the perpendicular courſe as along the ſurface of the earth, which however does not ſeem likely. The barometer ſtood then at 27 inches, and at their outſet it was at 30 inches on the ground. It would be eaſy, and it is ſurely deſirable to make experiments with cannon and ſtop watches on the velocity of aſcending, and if poſſible, deſcending ſound.

The ſtrength of the ſound perhaps depended on the ſtillneſs of the ſituation.

This reflection of ſound or echo is a ſubject of very great curioſity. There is perhaps no other inſtance in nature where ſo extended a wall of reflection can be had. I am diſpoſed to think that the apparent intenſity of the returned ſound may in ſome meaſure have depended on the perfect ſilence in which the ſpeakers were placed. In a ſtill night the centinels on the ramparts at Portſmouth may be heard at the Iſle of Wight, over a diſtance of five miles, and there are numerous inſtances of low ſounds, ſuch as the beating of a clock or watch, or the ſounds of footſteps being heard to conſiderable diſtances, when other ſounds do not act on the organ of ſenſe.*

Sudden riſe of the thermometer in deſcending.

In their deſcent to the earth they paſſed through various ſtrata of vapours, all of different temperatures, and at the inſtant the earth came in ſight, the thermometer ſtarted up through ſeveral degrees, probably becauſe they had quitted a cold maſs of vapour which obſcured their view, or perhaps becauſe the radiant heat of the earth's ſurface might at that moment have reached them unimpaired.

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* See a curious paper of M. Perrole on ſound, with the annotations thereon, in the firſt Vol. of our quarto Journal, page 411.—N.

XIII.

Letter from Mr. CUTHBERTSON, *containing Remarks on Mr.* WM. WILSON'S and *Mr.* HAUY'S *Experiments on the Electricity of Metals.*

To Mr. NICHOLSON.

London, 34, *Poland-Street*,
DEAR SIR, *April* 24, 1805.

Mr. Wilſon's experiments on electricity;

IN your valuable Journal for January, page 42, you have favoured us with a letter from Mr. Wilſon, containing ſome experiments which he calls, *exhibiting the electricity of metals*

of copper ſifted through zinc,

Mr. Wilſon ſays in his table of experiments, that copper filings ſifted through zinc is poſitively electrified: this is an *error* which I thought myſelf obliged to take notice of, as it is a direct contradiction to what I have aſſerted in *my examination of Sig. Volta's experiments, which he calls fundamental, and on which his theory of galvaniſm reſts; ſee this Journal, vol.* II. *page* 281; I have ſaid therein, that if a plate of zinc be ſeparated from a copper one, it will be poſitive, conſequently leave the copper negative if inſulated: whether this experiment be performed as I have therein mentioned, or according to Mr. Wilſon, the one reduced into filings, and afterwards ſifted through holes made in the other metal, the electricity excited muſt fall under the ſame denomination, the difference will be that the quantity of electricity excited will be more by Mr. W.'s method than mine, becauſe he has multiplied the ſeparations, a fact well worth notice.

is controverted by another experiment of contact and ſeparation.

Contact and ſeparation produce the electric ſtate,

Mr. W.'s chief object in view ſeemingly, is to prove that the ſeparation of metals is the cauſe of the electric fluid being excited and not touching. I cannot perceive that theſe experiments throw any light upon that ſubject, as both touching and ſeparating are here employed.

but neither act will do alone.

In my ſecond volume on electricity, publiſhed in Amſterdam, anno 1782, I have proved by experiment that neither *touching* nor *friction* ſeparately employed excites electric fluid, friction and ſeparating jointly employed, is a powerful exciter of electricity on glaſs; touching and ſeparating jointly employed on glaſs excite electric fluid in a ſlight degree, and only when the ſtate of the atmoſphere is favourable.

In

Hauy's obſ. on metallic ſubſtances, imperfect.

In the Philoſophical Magazine for Nov. 1804, page 120, we have Mr. Hauy's obſervations on the electricity of metallic ſubſtances: he does not inform us what ſhape his *ſilver* pieces were, or whether they were pure or with alloy; he however does not heſitate in pronouncing of it to be poſitively electrified by friction; ſo that it does not ſeem that he has entered very wide into the ſubject, or he would have perceived ſome remarkable changes to take place in that metal by friction, and particularly in coins. They will change from poſitive to negative, and *vice verſa*, without any viſible cauſe. If a dollar and a half-crown be ſtuck to the ends of two ſticks of ſealing-wax, and rubbed ſeparately upon woollen cloth, they will be found, after the friction, ſometimes poſitive and ſometimes negative, and ſometimes one poſitive and the other negative, without varying the manner of friction. If pure ſilver, or ſilver with different proportions of alloy, be melted down to a button, and uſed in that ſhape, or hammered flat, repreſenting coins, they are for the moſt part poſitive by friction. Theſe experiments upon metals are not new; I believe they were firſt begun by Mr. Henly, and inſerted in the Philoſophical Tranſactions; but I have not the data at hand, and I do not not remember that he had obſerved the above-mentioned property of this metal.

Singular effect of rubbing coins, &c.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Note on the New Planet Juno.

Planet Juno.

I HAVE not had any late account of the new planet announced at page 301 of our 9th volume (Dec. 1804) in a letter from M. Bode. The diſcoverer's name, who is Mr. Harding of Lilienthal, near Bremen, was not then mentioned.—For the preſent I give the following notes from the Journal de Phyſique, Thermidor laſt.

On the 5th of September, 1804, its right aſcenſion was 1° 52′; declination 0° 11′ north. M. Burckhardt obſerved it on the 23d of September at 359° 7′ and 4° 6′, whence he concludes that the duration of its revolution is five years and a half. Its inclination 21°; excentricity one quarter of its radius; mean diſtance from the ſun three times greater than that of the earth.

Its diameter could not be meaſured, but it appeared like a ſtar of the eighth magnitude. It ſeems nearly equal to that of Ceres, or the planet diſcovered by Piazzi.

*Diſcovery of Fluoric Acid in the Topaz.**

Saxon topaz analyſed by Vauquelin. Braſilian by Deſcotils.

IN the year 1797, M. Vauquelin analyſed the Saxon topaz, and found its conſtituent parts to be, ſilex 31, alumine 68. Mr. Deſcotils ſoon after examined the Braſilian topaz; but as there was a loſs of 18 *per cent.* in his firſt analyſis, and 12 in his ſecond, he did not think fit to publiſh the reſults of his labours at that time; and other circumſtances prevented him from purſuing the inquiry, as he had intended.

Klaproth diſcovered fluoric acid in the Saxon topaz.

Sulphuric acid incapable of expelling it.

Vauquelin treating the topaz with potaſh and ſulphuric acid expelled the fluoric.

Not long ago Mr. Klaproth wrote to Mr. Hauy, that he had found fluoric acid in the Saxon topaz. Mr. Laugier made ſeveral experiments to verify this diſcovery, but without ſucceſs. It is true in the analyſis with potaſh he found a deficiency of 16 per cent. but though he reduced the topaz to an impalpable powder, and did all he could to expel the fluoric acid from it by means of the ſulphuric, he was unſucceſsful. Mr. Vauquelin on his return applied himſelf to the ſame reſearch, and we here preſent the reſult of his labours. Not knowing what proceſs Mr. Klaproth had employed, he tried that which ſeemed to him moſt likely to ſucceed. He firſt heated the topaz with cauſtic potaſh in a ſilver crucible in the uſual way. After he had diluted the maſs with water, he introduced it into a retort, and poured on it ſulphuric acid. White fumes ſoon aroſe, and theſe, being collected, exhibited all the characteriſtics of fluoric acid combined with ſilex. The latter came almoſt wholly from the ſtone, as the retort was not perceptibly attacked by the acid.

The Braſilian topaz gives the ſame reſult.

Topaz a true alumino-ſiliceous fluate.

The ſame experiment with the Braſilian topaz gave the ſame reſult; and there is every reaſon to believe, that the Siberian, which Mr. Vauquelin is now analyſing, will afford the ſame products. Thus we may now conſider this gem as a ſiliceous compound, conſiſting of fluoric acid, alumine, and ſilex, or a true alumino-ſiliceous fluate; and the diſcovery muſt be conſidered as of the higheſt importance in mineralogy.

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* Bulletin des Sciences, No. XC. p. 282, Sept. 1804.

Circumſtance that miſled Vauquelin in his firſt analyſis.

Mr. Vauquelin next inquired what might be the circumſtance, that led him into an error when he firſt analyſed the topaz; and he imagines, that it was his treating the alcaline maſs with muriatic acid, inſtead of employing the ſulphuric; and that probably not heating it to a ſufficient degree to expel the fluoric acid, from fear of decompoſing the muriate of alumine, he precipitated the fluoric acid combined with the alumine, when he added ammonia to ſeparate the alumine from its muriatic ſolution.

Examination of Crude Platina, by Meſſrs. Tennant *and* Wollaston.*

Platina.

THE editors of the *Annales de Muſeum* obſerve, that platina, when taken from the mine, appears from the experiments of Deſcotils, Fourcroy, and Vauquelin, to be mixed with iron, chrome, and other metals, among which may be one or two that are new. Mr. Tennant has given names to two, and Dr. Wollaſton to two others; but the French journaliſts congratulate their countrymen for being leſs haſty, and waiting till they are aſſured theſe new metals really exiſt:

Prize Queſtions in France.

Prize queſtions reſpecting wool; and an inſect on the colſa.

The Society of Agriculture and the Arts, in the department of the North, propoſes for the ſubjects of two prizes, which ſhall be adjudged, the firſt in the firſt of Fructidor, in the year XIII. and the ſecond in the firſt fortnight of Fructidor, in the year XIV. the two following queſtions:

Firſt Queſtion.—" What method of propagating, rearing, feeding, and houſing the ſheep of the race now exiſting in the department of the North, ought to be followed in this department, to obtain wool from theſe animals, equal in quality to the beſt wools from Engliſh ſheep?"

Second Queſtion.—An inſect known in the country under the improper denomination of *puceron*, has this and ſeveral preceding years attacked and deſtroyed the greater part of the flowers of the colza.—" What is this inſect? Under what generic and ſpecific name have the moſt celebrated naturaliſts deſcribed it? What is its life, either in the ſtate of coleoptera in which it is found on the colza, or in the ſtate of larva? What natural enemies, what artificial means of deſtruction can be oppoſed to it in either ſtate? Generally, what care can preſerve the colza from its ravages?"

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* Bulletin des Sciences, No. XC. p. 234, Sept. 1804.

Each of theſe two prizes will be a gold medal of the value of 150 francs.

Ruſſian Circumnavigation.

Ruſſian voyage round the world.

By a courier, expedited by the governor of Kamſchatka, news was received at Peterſburgh on the 21ſt of December, that the veſſels belonging to the expedition round the world under the orders of M. de Kruſenſtern, had arrived, on the 26th of July laſt, at the harbour of Peter and Paul, in Kamſchatka, and that, up to this time M. Kruſenſtern had not loſt a ſingle man of his retinue, nor had he any ſick in the ſquadron. In his voyage he had viſited the Marqueſas and Sandwich Iſlands. M. Kruſenſtern purpoſed failing for Japan, towards the end of Auguſt.

Europeans found [illegible] the Marqueſas, [illegible]ho had forgot[illegible]n their own [illegible]guage.

At one of the Marqueſas, where the veſſels ſtopped a few days, he had taken on board a Frenchman and an Engliſhman, to bring them back to Europe. Captain Kruſenſtern ſays he had not yet been able to diſcover how theſe two individuals had come to this iſland; both of them having completely forgotten their original language. He thought however that he could comprehend that they had arrived there on board an American veſſel which had been ſhipwrecked on the coaſt. The Frenchman ſpeaks the language of the iſlanders very well, and has adopted all their cuſtoms, habits and manner. There is no doubt that they will ſoon recover the uſe of their language, in a daily intercourſe with Europeans; and that they will then be able to give an account of their adventures, as well as information reſpecting the iſlanders, among whom they have lived ſo long. At leaſt, this is expected with impatience.

Geological Journey from the Academy at Warſaw.

Journeys of diſcovery in mineralogy and natural hiſtory.

The Society of the Friends of the Sciences at Warſaw have charged two of its members, M. Carteau and Stacio, to undertake a mineralogical and phyſical expedition into the Carpathian mountains. Another member of the ſociety has already examined the eaſtern part of them, with relation to mineralogy, geogony and oryctognoſy. At this time he is travelling over the mountains in the interior of Auſtria, from whence he will proceed to upper Italy, and to the Alps of Switzerland. When this journey is completed, he will undertake another to Caucaſus.

Lalande's Propoſal of a new Scale of the Thermometer.

New ſcale of thermometer

M. de Lalande propoſes to adopt a thermometer-ſcale, which ſhall remedy all the inconveniences of thoſe now in uſe. His mean point is taken from the natural ſtate of the globe, which he fixes at 9½ degrees of Reaumur's thermometer, and he takes the 10 millionth part of the volume of mercury for the meaſure of a degree. Among the advantages of this inſtrument he reckons a ſimplification of expreſſion, which will give a facility to comprehending what was before without meaning to the public. For example, the degree of heat of common ſummers, and the degree of cold of our mean winters, will be both expreſſed by 30: The degree of 40 will indicate a hot ſummer and a ſevere winter, &c.; another advantage will be derived from the ſmaller interval of the degrees, which will remove the neceſſity of having recourſe to fractions in the greater number of obſervations. The boiling point of water is + 132°.5 of the propoſed thermometer, and – 74°.4 is the point of the congelation of mercury. Ice melts at – 17°.9 and – 44°.2 is the zero of Fahrenheit.

Two Kinds of Honey.

Prouſt on honey.

In a note to Dr. Delametherie, Prouſt announces the diſcovery of two kinds of honey; the one liquid, the other dry, not deliqueſcent, cryſtallizable in its manner, and leſs ſaccharine than ſugar: they are ſeparated by ſpirit of wine, to which end granulated honey muſt be operated on.

J. de Phyſique.

Palladium.

Attempts to form palladium with mercury and platina.

Experiments made by Meſſrs. Roſe and Gehlen, and others by Richter, to obtain palladium, are given at length in the Journal of Chemiſtry, publiſhed in German by Klaproth and Richter.

The

Theſe philoſophers followed Mr. Chenevix's proceſs with great care, but did not produce that metallic body. In the precipitation of muriate of mercury and muriate of platina, they had a black powder, which always afforded the metals ſeparate from each other.

Richter was not more ſucceſsful. He verified that the green ſulphate of iron does not decompoſe either the muriate of mercury or that of platina. The other facts he obſerved were to the ſame effect as thoſe of Roſe and Gehlen. He always found the mercury of his precipitate ſeparated from the platina by heat.

Traité élémentaire d'Hiſtoire Naturelle, &c. *An elementary Treatiſe on Natural Hiſtory, by* A. M. CONSTAT DUME'RIL: *Compoſed by Order of Government for the Uſe of the National Lyceums,* 1 *Vol.* 8vo. *Paris* [*].

Elementary works on natural hiſtory very defective.

Mr. D. has purſued a better method.

There is not one of the ſciences, the elementary works of which have been ſo long neglected as natural hiſtory. Sometimes this title has been given to collections of tales fit only to amuſe children, but not calculated to make them acquainted with nature as a whole, and with the progreſs of the ſcience: at other times authors have entered into diſcuſſions too abſtruſe, or contented themſelves with a mere nomenclature, always dry and ſterile to beginners, to whom names give no idea of objects with which they are unacquainted. Mr. D. has preſerved a juſt medium between theſe extremes, while he obſerves an accurate and methodical arrangement. He makes us acquainted with the whole of the productions of nature, and the method of ſtudying and claſſing them, chooſing for examples in every ſection ſuch as are moſt remarkable for their uſes or ſingularity; he continually excites the attention and curioſity of his pupils; and he preſents to them a number of facts neceſſary to be known.

His arrangement proceeds from the moſt ſimple things to the more complex.

In this work Mr. D. has adopted an arrangement, the reverſe of what is uſually employed in books of natural hiſtory; that is, he always proceeds from the moſt ſimple to the moſt complex. He begins with unorganized ſubſtances, proceeds hence to plants, and laſtly to animals; and in theſe he commences with zoophytes, and ends with man. This arrangement has the advantage of inſtilling ideas gradually into the mind of the ſcholar, and avoiding a number of repetitions and anticipations. The hiſtory of organiſed ſubſtances gives him an idea of bodies unmixed with any other ideas: that of vegetables ſhows him organization and life in their moſt ſimple ſtate: and theſe he perceives gradually become more complicated as he aſcends through the different claſſes of the animal kingdom, ſo that the hiſtory of each claſs is but little more than an expoſition of the organs and faculties it enjoys beyond thoſe of the preceding.

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[*] Bulletin des Sciences, No. 90, p. 236, Sept. 1804.

Its advantages.

Though the diſcuſſion of any new idea ſeems contrary to the eſſence of an elementary work, it is obvious, that ſuch a work cannot be well executed but by a man capable of conſidering the whole of a ſcience in its proper point of view. In this reſpect the naturaliſt will here read with pleaſure the article of general obſervations placed at the head of each part: he will diſtinguiſh the hiſtory of inſects, which Mr. D. has treated after a new plan: and he will notice the chapter on man, in which the author diſplays the phyſical characters that diſtinguiſh man from brutes, and the conſequences reſpecting his manners that ariſe from his very ſtructure. This chapter may be conſidered as the connecting link between phyſics and metaphyſics.

Mr. D. has treated the inſects on a new plan.

Elemens de l'Art de la Teinture, &c. or *Elements of the Art of Dying; with a Deſcription of the Proceſs of Bleaching by the oxigenated Muriatic Acid. Second Edition*: *by* C. L. *and* A. B. Berthollet. *Paris*.

This new edition of a work of the firſt merit and celebrity is ſpoken of in the Foreign Journals, as being conſiderably improved by the former author and his ſon. The great perſpicuity and order which are ſeen in all the productions of this eminent chemiſt, and his own unremitted labours in the ſcience, are a ſure guarantee to the ſame effect.

Berthollet on dying.

Conſiderations on Organized Beings. By J. C. Delametherie.

The ſcience of natural hiſtory is indebted to M. Delametherie for ſeveral intereſting works which contain ideas of advantage to the progreſs of human knowledge. Beſides the

Delametherie on organized beings.

Journal de Phyſique, &c. of which he is the editor, he has publiſhed a Theory of the Earth; a Treatiſe on Man; different Phyſiological Views of the animal and vegetable Kingdoms, on Vital Air, &c. In this new work the author compares the ſtructure of animals and plants, and applies to vegetables the different ſyſtems or apparatuſes of organs and vital functions, which X. Bichat has propoſed in his General Anatomy. M. Delametherie has benefitted by the experiments and labours of ſeveral philoſophers of merit. All, perhaps, may not adopt ſome of the opinions he offers, the conqueſt of minds being ſtill more difficult than that of hearts; but they, at leaſt, deſerve examination, and may lead to unexpected reſults.

Bibliotheque de Sonini.

To CORRESPONDENTS.

My beſt acknowledgements are due to Mr. D. *who has favoured this work with a preciſe ſtatement of the elucidation of Mr.* BOSWELL'S *ſecond propoſition; but he will perceive that the paper of Mr.* GOUGH, *which was already printed when his letter came to hand, has rendered it unneceſſary.*

I believe the readers of this Journal will think with me, that the diſpute between C. L. *and Mr.* E. WALKER *has proceeded at leaſt as far as the intereſts of ſcience demand. In a letter from* C. L. *before me, I have ſuppoſed the following explanatory ſentences to afford no new ground for diſcuſſion, and therefore extract them, and hope the buſineſs will end here.* C. L. *ſays:*

"*I cannot conceive how expreſſions ſo plain could be miſunderſtood, and hope you will permit me to endeavour to make them plainer if poſſible.* I have denied Mr. WALKER'S facts; *that is, I have denied the* truth of *his narrative reſpecting* certain ſuppoſed facts, *and I have pointed out an eaſy way of convincing me and the world that he is not deceived; namely, that you ſhould repeat his experiments, and ſee if what he aſſerts be true. But Mr.* WALKER declines examining the remainder of my letter, *which is an indiſtinct, though ſufficiently clear way of ſaying, that he does not chooſe to riſk his ſuppoſed facts by putting them to ſuch a trial.*"

Mr. Dodds gun Lock.

Fig 3

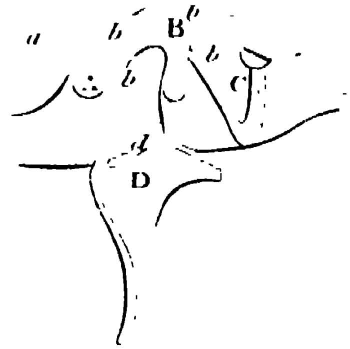


Fig 1

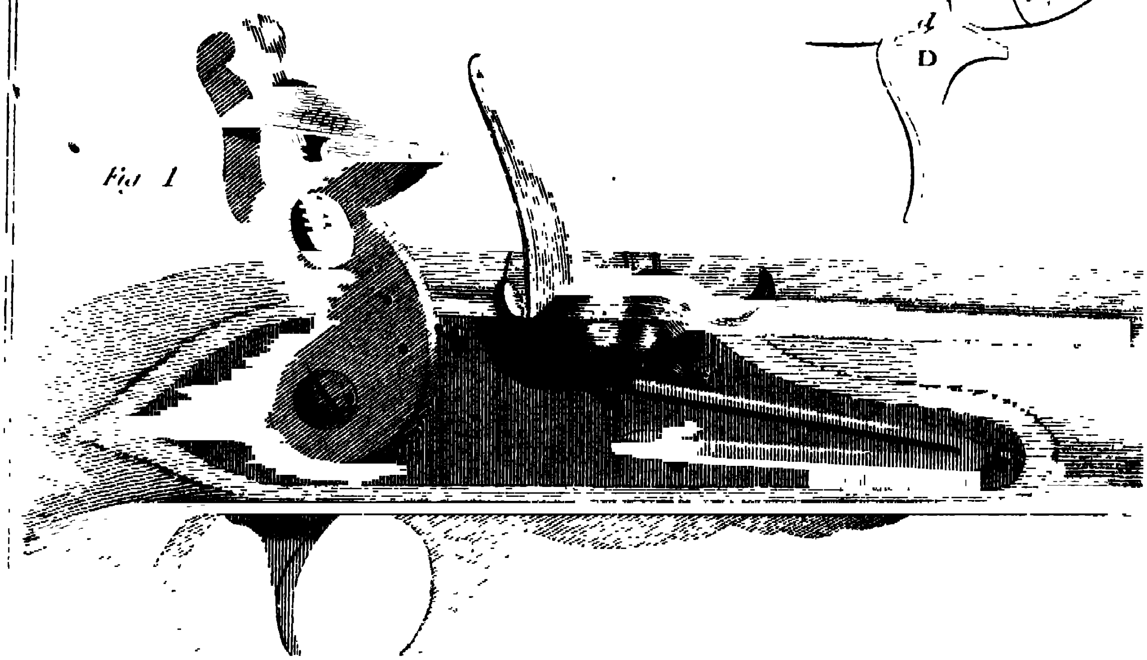


Fig 2

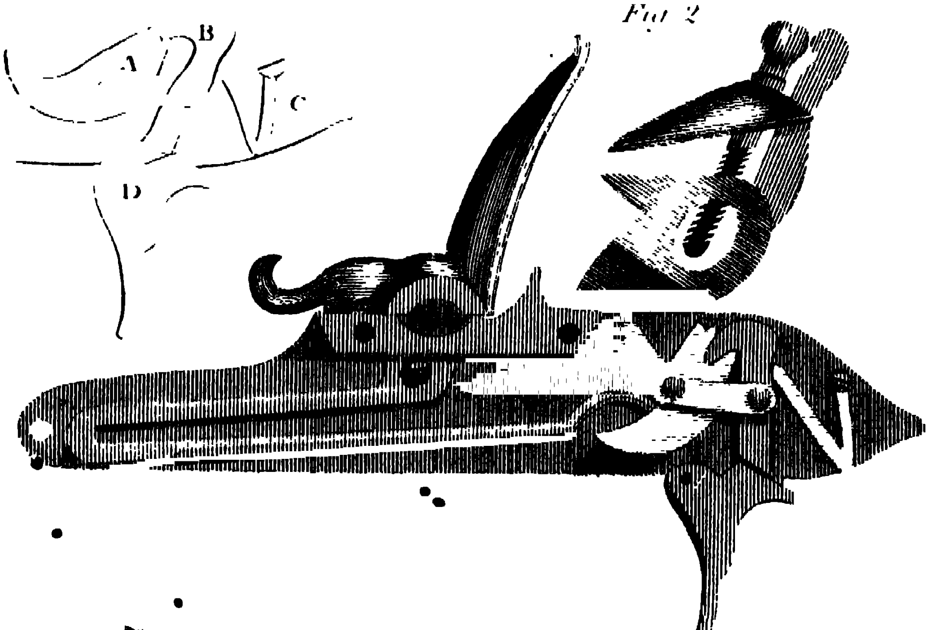


Fig 6.

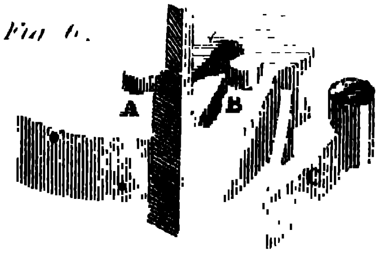
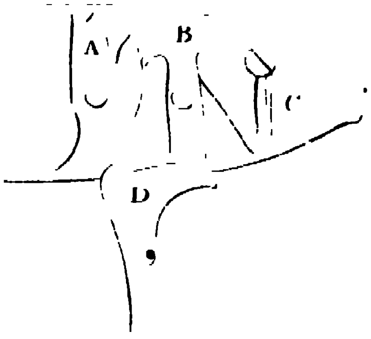
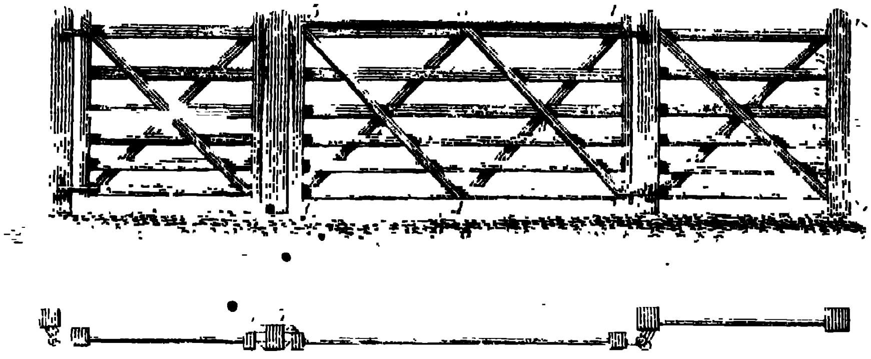


Fig 5.



Mr. Waddell's improved Field gate

Fig. 1

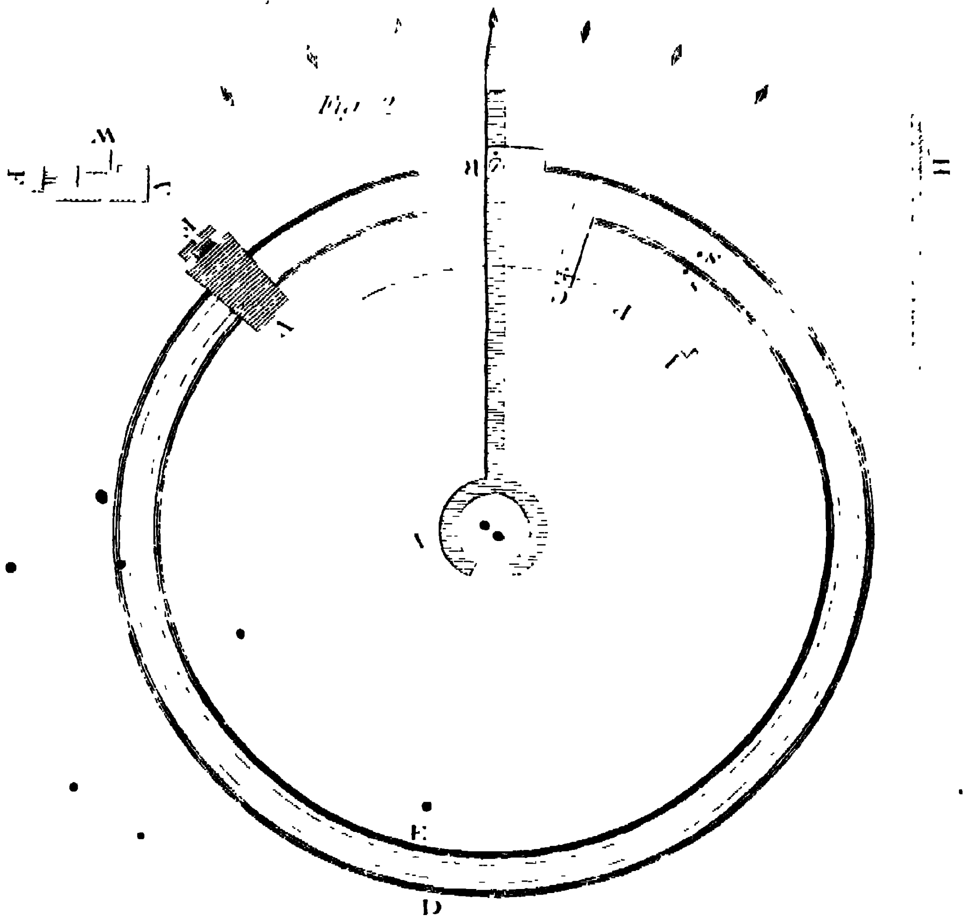


Scale [illegible] feet

Mr. Waddell's [illegible]

for [illegible] Stone [illegible]

Fig. 2



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Bavarian Method of evaporating Salt Water

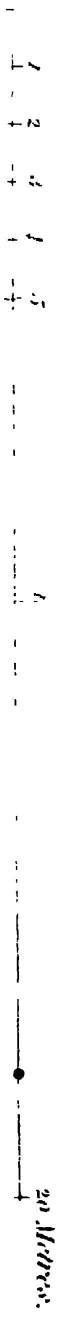


Fig. 3

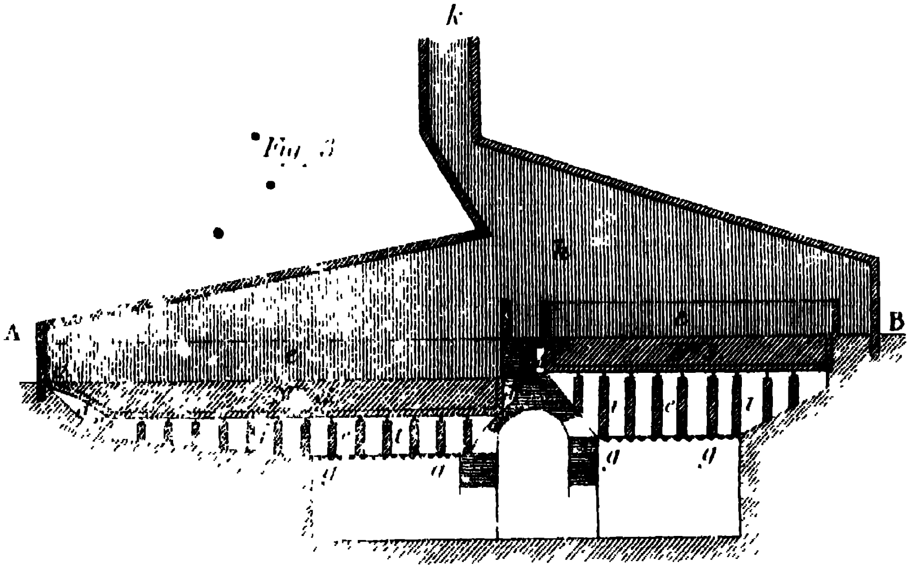


Fig. 1

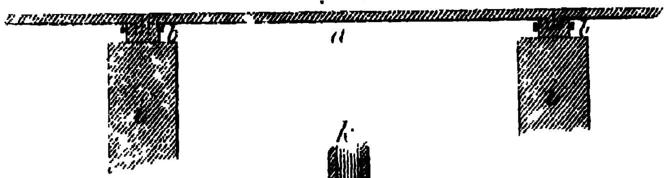
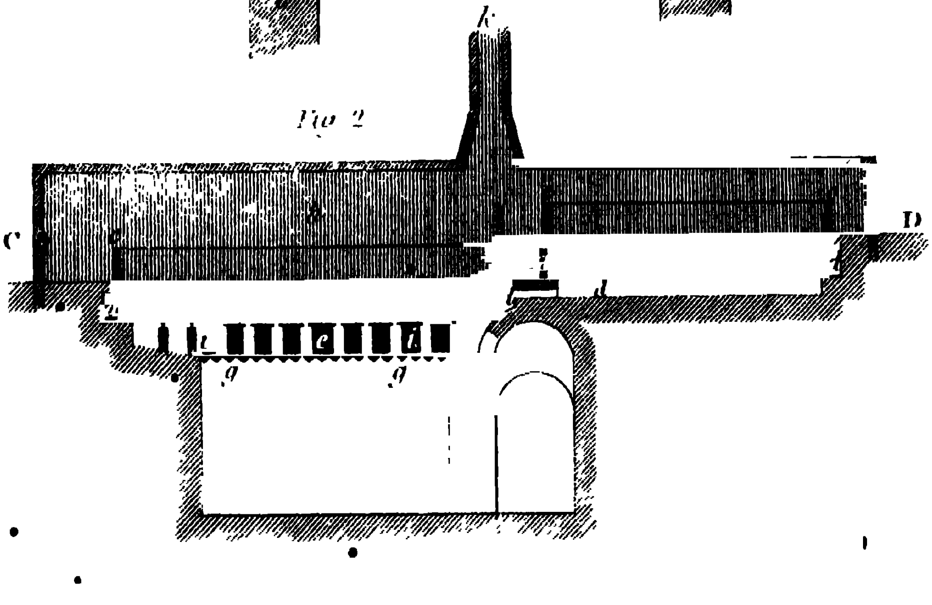
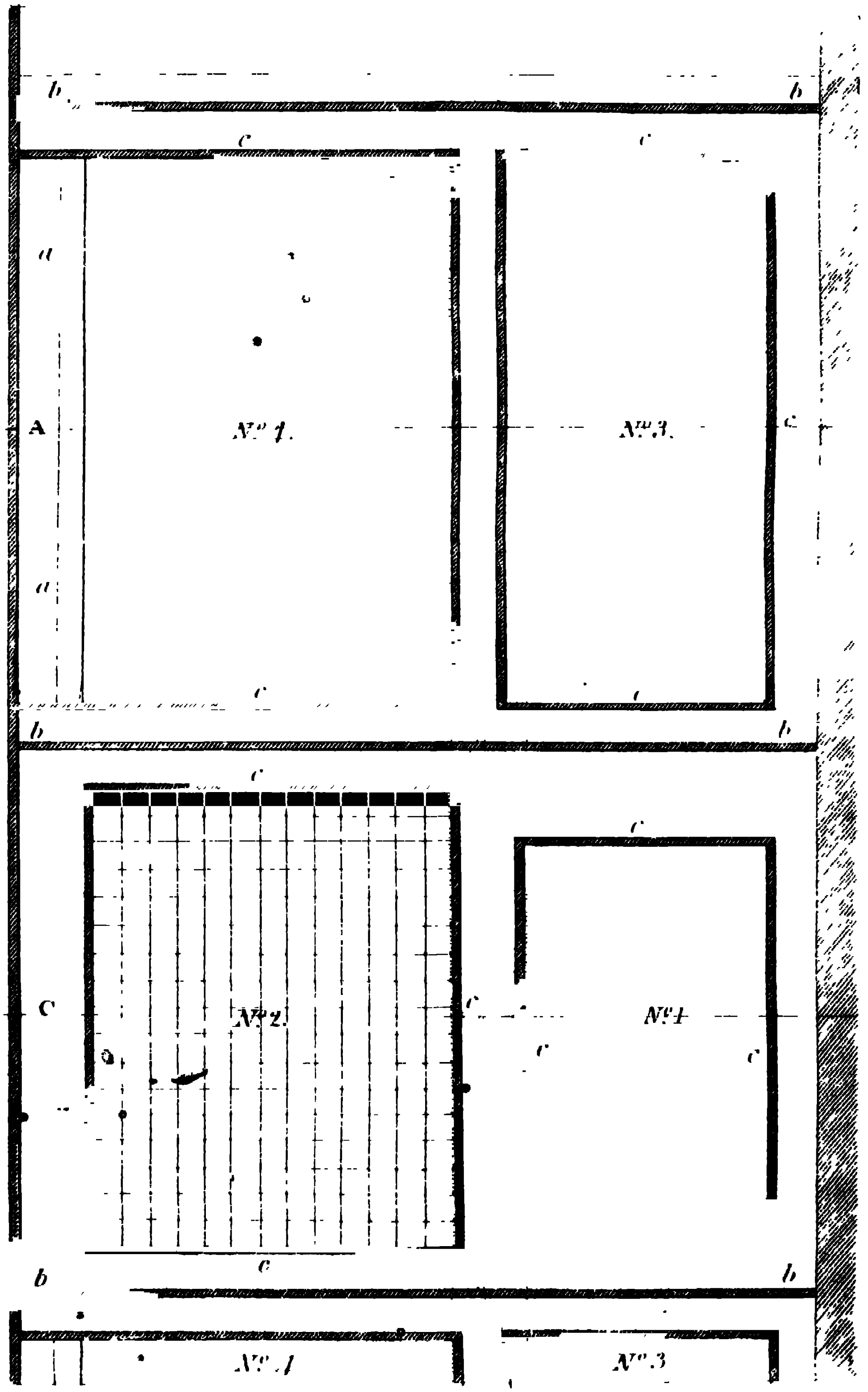


Fig. 2



Mutlow sc. Russ. Pl.

Fig. 1



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1805.

ARTICLE I.

Experiments on the Gaſes obtained by the deſtructive Diſtillation of Wood, Peat, Pit-Coal, Oil, Wax, &c. with a View to the Theory of their Combuſtion, when employed as Sources of artificial Light; aud including Obſervations on Hydro-Carburets in general, and the Carbonic Oxide: By Mr. WILLIAM HENRY. *(Communicated by the Author.)*

Proceſs of Mr. Murdoch to illuminate by gas from coal.

Neither hidrogen nor carbonic oxide give any notable light; but the gas from coal is nearly equal to oil.

THE gas obtained by the deſtructive diſtillation of pit-coal, has become an object of conſiderable intereſt and importance, in conſequence of its ſucceſsful application (by Mr. Murdoch, of Soho, near Birmingham *) to the purpoſe of affording light. Having conſtructed an Argand's lamp laſt winter, with the view of effecting the combuſtion of the gas on Mr. Murdoch's plan, I made previous trials with pure hydrogen gas, with carburetted hydrogen obtained by paſſing water over ignited charcoal, and with the carbonic oxide; but found that each of them burned with ſo trifling a production of light, as to be altogether unfit for the purpoſe of illumination; while the light evolved by the gas from coal was little, if at all, inferior to that from good ſpermaceti oil. So eſſential a difference in the combuſtion of theſe gaſes, induced me at firſt to believe, that the gas from coal owes its illuminating property to ſomething mechanically ſuſpended in it; but I was afterwards ſatisfied, that though it contains, when recently prepared, much that is ſubſequently depoſited, yet that its quality of burning with a bright and compact flame belongs to it, though certainly with conſiderable diminution, as a permanent gas, after the ſeparation of all condenſible matter. It appeared, therefore, worthy of inveſtigation to determine, on what the ſuperior fitneſs of the gas from coal, for evolving light, depends; and to connect the theory of its combuſtion with that of other ſubſtances, commonly employed as ſources of artificial light. With this view, numerous comparative experiments were made on the rapid combuſtion of this gas with oxigenous gas in cloſe veſſels, and alſo on that of other inflammable gaſes; and their compoſition may be inferred from the products of theſe experiments, the principal reſults of which are contained in the following table :

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* See the ſtatement of Mr. M's claim to the diſcovery in the poſtſcript.

This laſt gas burns with much (though diminiſhed) light after it has depoſited its condenſible matter.

Products by burning the gas from coal with oxigen and alſo other gaſes.

Table of reſults.

Kind of Gas.	Oxigen Gas required to ſaturate 100 Meaſures.	Carbonic Acid produced.
Pure hydrogen,	50 to 54	
Gas from moiſt charcoal,	60	35
——— wood (oak)	54.	33
——— dried peat,	68	43
——— coal, or cannel,	170	100
——— lamp-oil,	190	124
——— wax,	220	137
Pure olefiant gas,	284	179

Now if it be aſſumed (which I believe is as nearly as poſſible the fact) that in the formation of each meaſure of carbonic acid, in the above experiments, an equal volume of oxigen gas is employed*, we ſhall learn, by deducting the numbers in the

If the meaſure of carbonic acid produced, be deducted from the whole oxigen employed, the remainder will expreſs the meaſure of oxigen which was employed in burning the hydrogen of the gas; and this laſt doubled will give the third column from the correſponding one in the ſecond, what proportion of the conſumed oxigen has been allotted to the ſaturation of the hydrogen of each hydro-carburet. Thus, for example, in the combuſtion of the gas from coal, 70 parts of oxigen have diſappeared, beſides that which has entered into the carbonic acid; and, ſince each meaſure of oxigen ſaturates two of hydrogen gas, the gas from coal muſt contain, in 100 meaſures, a quantity of hydrogen which, expanded to its uſual elaſticity, would occupy 140 meaſures. By a ſimilar mode of eſtimation, the quantity of hydrogen in other ſpecies of inflammable gas may be aſcertained; *viz.* by ſubtracting the number in the third from the correſponding one in the ſecond column, in each inſtance, and doubling the remainder.

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* Mr. Cruickſhank takes it for granted, as the baſis of his calculations, that in forming ſix meaſures of carbonic acid, ſeven meaſures of oxigenous gas are employed. This proportion I believe to be over-eſtimated. Dr. Prieſtley obſerves, (on Air, 2d Edition, III. 377), "I heated 8¼ grains of perfect charcoal in 70 ounce-meaſures of dephlogiſticated air, when it ſtill remained 70 oz.

meaſure of hydrogen originally preſent.

Theſe facts explain the greater illumination from the coal gas. The light is greater the more oxigen is required for the combuſtion.

The above experiments ſufficiently explain why the gas from coal evolves ſo much more light, during combuſtion, than either hydrogen or the hydro-carburet from moiſt charcoal, becauſe, in an equal volume, it includes, in its compoſition, above thrice the quantity of inflammable matter preſent in hydrogen gas, and nearly thrice as much as is contained in the gas from moiſt charcoal. The appreciation of the degree of combuſtibility of each gas, by the quantity of oxigen required for its ſaturation, entirely agrees, as might naturally be expected, with that founded on the phenomena of ſilent combuſtion in an Argand's lamp; for each gas ſeemed to me to evolve light, as nearly as could be judged, in proportion to the quantity of oxigen conſumed by its detonation in a cloſe veſſel. Above all others, the olefiant gas * is decidedly entitled to rank, by the ſplendor and beauty of the light which it yields; and the violence of its detonation, when fired with a mixture of oxigen gas, alſo ſurpaſſes that of every other inflammable gas. By exploding only .03 of a cubic inch with .17 of oxigen gas, a ſtrong glaſs tube was

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m.; but, after waſhing in water, was reduced to 40 oz. m." In this experiment one grain and ¼ of charcoal was conſumed, and 30 oz. m. of carbonic acid were generated, without any change in the volume of the oxigenous gas.

* A full abſtract of the memoir of Meſſrs. Deiman, &c. on this intereſting gas, may be ſeen in the 1ſt Vol. of the 4to Series of this Journal. Its characteriſtic property is that of being rapidly converted into oil, by contact with oxigeniſed muriatic acid gas.

ſhattered with violence; and a Volta's eudiometer, ¼ of an inch in thickneſs was burſt by leſs than a cubic inch of a mixture of the two gaſes. The ſpecific gravity of the inflammable gaſes, when perfectly freed from carbonic acid, is another competent teſt of their fitneſs as ſources of light. Thus the ſpecific gravity of the gas from moiſt charcoal, (common air being 1000) according to Cruickſhank is 480; of the hydro-carburet from alcohol 520, and of the olefiant gas, as determined by the Dutch chemiſts, 909.

Specific gravity of an inflammable gas (freed from carbonic acid) is a teſt of its fitneſs to afford light.

It is probable that the inflammable gaſes are mixtures of few ſimple gaſes.

From the limitation to the proportions, in which bodies in general, having a ſuſceptibility of chemical union, are capable of combining, it ſeems to me reaſonable to infer, that carbon and hydrogen do not unite in all poſſible proportions, forming ſo many diſtinct compounds; but that the various inflammable gaſes are mixtures of a very few ſimple ones. Of thoſe at preſent known, pure hydrogen gas; the carburetted hydrogen, which by combuſtion affords an equal bulk of carbonic acid, and conſumes twice its bulk of oxigen; the carbonic oxide; and the olefiant gas, it will appear, may be traced in the mixed gaſes comprehended in the forgoing table. The gas from coal I apprehend to be principally hydro-carburet, with perhaps ſome portion of carbonic oxide, the preſence of which laſt is rendered probable, becauſe the gas from coal is ſaturated by leſs than twice its bulk of oxigen, though it gives an equal volume of carbonic acid. Now the gas from marſhes, which, with Mr. Cruickſhank, Mr. Dalton finds to be hydro-carburet, contaminated with about 20 *per cent.* azotic gas, conſumes, making allowance for this adulteration, double its volume of oxigen gas; and ſince the gas from coal requires a leſs proportion than this of oxigen, and yet gives an equal product of carbonic acid with that from marſhes, it is fair to preſume, that it muſt previouſly have contained ſome oxigen, which, after waſhing it with lime-water, can ſubſiſt in no other ſtate than that of the carbonic oxide.

Gas from coal appears to be hydro-carburet with perhaps ſome carbonic oxide.

Gas from ignited charcoal and water is probably carbonic oxide with hydrogen and a little hydro-carburet.

The gas obtained by decompoſing water over ignited charcoal, is moſt probably a mixture of carbonic oxide with hydrogen gas, and perhaps a little hydro-carburet. On no other preſumption can the reſults of its combuſtion be explained; ſince the quantity of oxigen required in ſaturating 100 meaſures is only ten more than are conſumed by 100 meaſures of pure hydrogen, though 35 m. of carbonic acid, containing at leaſt

35 of oxigen, are found after combuſtion. Now, according to Mr. Cruickſhank, theſe 35 m. of carbonic acid, if formed from carbonic oxide, would require only 15 additional meaſures of oxigen gas, which is not very remote from the truth. Reaſoning in the ſame mode, the gaſes from wood and from peat will appear to be mixtures in different proportions of two at leaſt of the above-mentioned, viz. hydrogen, and the carbonic oxide. The reſults of the combuſtion of the gaſes from wood and peat evince that they differ conſiderably from that obtained from coal; and contain much leſs uncombined inflammable matter. Another circumſtance of diſtinction alſo is, that, before being waſhed with lime-liquor, the gas recently prepared from wood or from peat, contains from $\frac{1}{4}$ to $\frac{1}{3}$ its bulk of carbonic acid; whereas the gas from coal loſes by this ablution only from $\frac{1}{27}$ to $\frac{1}{13}$. * In my firſt experiment, I found a large admixture of azote in all theſe gaſes; but this afterwards proved to be accidental and not eſſential; ſince by careful diſtillation in glaſs retorts, of the ſubſtances that afforded them, the gaſes were obtained entirely free from this contamination.

Gaſes from wood and from peat contain leſs of uncombined inflammable matter than the coal gas.

The gaſes obtained by the deſtructive diſtillation of oil and of wax, it may be obſerved in the table, conſume conſiderably more oxigen than the gas from coal. This circumſtance firſt led me to ſuſpect that they might poſſibly be mixtures of the olefiant gas with carburetted hydrogen; and on applying the oxygenized muriatic acid gas, this ſuſpicion was fully verified. One meaſure of the gas from oil with one of the oxygenized gas, were reduced ſpeedily to 1¾; a like diminution was produced in the gas from tallow; and that from wax had its bulk ſtill further contracted, only 1½ m. being left by ſimilar proportions.

The gaſes from oil and from wax are pure hydro-carburets, except that the firſt contains one-eighth and the latter one-fourth of olefiant gas.

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* The condenſible products alſo of coal probably differ from thoſe of wood and peat. If an intermediate veſſel be placed for their reception, it emits, after the diſtillation, a ſtrong ſmell of ammonia. This was long ago obſerved by Lord Dundonald, who enumerates, among other products of coal, the volatile alkali (ſee a pamphlet "on the Uſes and Qualities of Coal Tar," publiſhed by his Lordſhip in 1785.) This production of ammonia I have not obſerved from peat or wood; nor do I find it mentioned in a Hiſtory of Peat, including the reſults of its diſtillation, &c. publiſhed in the ſecond vol. of the Edinburgh "Eſſays Phyſical and Literary."

In order to aſcertain how much of the diminution was owing to the condenſation of olefiant gas, the proportion of oxygenized acid, required for the ſaturation of that gas, was carefully aſcertained. After ſeveral trials, it was found that 3 m. of the oxygenized acid with $2\frac{1}{2}$ pure olefiant gas, prepared according to the Dutch chemiſts, left only 0.15 m. of common air derived from the veſſels. It appears, therefore, that the gas from oil and from tallow contains about $\frac{1}{8}$, and that from wax $\frac{1}{4}$ olefiant gas, the reſt being pure hydro-carburet.

Hence it is ſeen why waſhing diminiſhes the product of carbonic acid afforded by burning an hydrocarburet. For it takes away the olefiant gas.

The reſults of theſe experiments, in connection with a fact communicated to me by Mr. Dalton, explain ſome circumſtances obſerved by Mr. Cruickſhank, for which that ingenious chemiſt was at a loſs to account, viz. the great variation in the products of carbonic acid, obtained by burning the ſame hydrocarburet when waſhed and when unwaſhed, or when long kept in contact with water; though the gas, when originally procured, was perfectly free from carbonic acid. The oleſiant gas, Mr. Dalton has aſcertained, is far more abſorbable by water than other ſpecies of hydrocarburet, viz. in the proportion nearly of $\frac{1}{8}$. Now the gas from camphor, I find to contain much oleſiant gas, and indeed this might have been inferred from Mr. Cruickſhank's own ſtatement, who obſerves that this gas, by admixture with oxygenized muriatic acid, undergoes a conſiderable diminution of bulk. The pure hydrocarburet, on the contrary, I have never ſeen at all condenſed by contact with this gas, in the rapid manner obſervable in olefiant gas or mixtures containing it; though, by confinement together for ſome hours, they are mutually decompoſed into common muriatic acid, carbonic acid, and water. The hydrocarburets from ether and alcohol alſo contain olefiant gas; and this, I apprehend, will be found to be the fact with all inflammable gaſes, which by combuſtion give more than their own bulk of carbonic acid. The variable products of carbonic acid, obtained in Mr. Cruikſhank's experiments, from equal quantities of different hydrocarburets, cannot, therefore, be conſidered as denoting ſo many diſtinct ſpecies of carburetted hydrogen; but as owing to the admixture with this of various proportions of olefiant gas.

Hydrocarburets from ether and alcohol alſo contain ol. gas; which is the general cauſe of the difference in carb. acid products by Mr. Cruikſhank.

Error of that author reſpecting the conſtitution of carburetted hydrogen gaſes.

It will not, I am perſuaded, be regarded as indicating a wiſh to detract, in the ſmalleſt degree, from the credit due to Mr. Cruikſhank, whoſe memoirs on the hydrocarburets and carbonic oxide I eſtimate among the moſt ingenious, and generally ſpeaking, the moſt ſatisfactory examples of chemical reſearch, if I obſerve that the part of his table (vol. V. p. 8 of the 4to ſeries of this Journal) which relates to the conſtitution of the carburetted hydrogen gaſes, I conſider as entirely erroneous. To excite ſtrong ſuſpicion of the accuracy of the proportions aſſigned to theſe gaſes, it is ſurely ſufficient that one of them (the gas from moiſt charcoal) is ſtated to contain in 100 cub. in. = 14½ grains, no leſs than 9 grains of water, a proportion abſolutely inconceivable; and the ſame objection applies, in a leſs degree, to the other caſes. Now in 100 cubic inches of the muriatic acid gas, I found the abſolute quantity of combined water to be only 1.4 gr. (Phil. Tranſ. 1800); and it is rendered highly probable, by the experiments of Clément and Deſorme (*Ann. de Chim.* XLII. 121) that all gaſes contain the ſame quantity of water. In the inſtance of the gas from charcoal, Mr. C. was moſt probably miſled, by not having ſuſpected the preſence of the carbonic oxide; and the correction is to be made as follows. One hundred cubic inches (=14½ grains) combined with the proper quantity of oxigen, gave 19 grains of carbonic acid, containing very nearly 4 grains of carbon; and ſuppoſing the carbon in the gas before combuſtion to have been in the ſtate of carbonic oxide, it would be combined with about 9 grains of oxygen, and would conſtitute 13 gr. or 43½ cub. in. of carbonic oxide.— There remains then only 1½ grain, of the 14½ ſubmitted to experiment to be accounted for, which is very exactly made up by the reſiduary 57½ cub. in. of hydrogen gas, taking the weight of 100 cub. in. of hidrogen to be 2.6 gr. The water contained in the gas may, I think, be ſet out of the queſtion; for it muſt be recollected that the product of the combuſtion is in part aeriform; and it may be conſidered as a tolerable approximation to the truth, that the gas from charcoal contains, in 100 inches, 43 of carbonic oxide, the remainder being principally hydrogen gas.

With reſpect to the preſence of hydrogen in the carbonic oxide, which has been a topic of controverſy, neither the fact nor the negative can, I think, be at preſent with certainty affirmed. If however any hydrogen be contained in it, I ſhould deem it an accidental and not an eſſential ingredient, and am [illegible] opinion that, if preſent at all, it exiſts in the ſtate of hydro-

Carbonic oxide does not contain combined hidrogen, &c.

gen gas; for I find that the carbonic oxide is not expanded by electrical diſcharges, which would aſſuredly happen if the carburetted hydrogen were one of its conſtituents, or accidentally mixed with it.

Elucidation of the theory of lamps, &c.

The oil, &c. is decompoſed in the wick,

—into olefiant and carburetted hydrogen gaſes, which are then burned. Whence the fitneſs of any fuel to give luminous flame may be known from its deſtructive diſtillation.

To return to the theory of lamps, &c. it is proved by the preceding experiments, that the ſubſtances ordinarily employed as ſources of artificial light, viz. oil, tallow, and wax, afford when ſubmitted to an increaſed temperature, much olefiant gas; and it has been juſtly obſerved by the editor of this Journal (4to ſeries I. 71) "that the wick of a lamp or candle ſurrounded by flame is exactly in the ſituation of a body expoſed to deſtructive diſtillation in a cloſe veſſel." In this caſe the ſeries of capillary tubes compoſing the wick, ſerve perhaps preciſely the ſame office as a tube horizontally diſpoſed in a heated furnace, through which an inflammable liquid is tranſmitted. The fuel previouſly melted, is drawn up into theſe ignited capillary tubes, and there reſolved into olefiant and carburetted hydrogen gaſes, from the combuſtion of which gaſes, and not merely of a condenſible vapour, it appears to me that the illumination chiefly proceeds. Hence it is not improbable, that the proportion of olefiant gas and hydrocarburet, obtained by the diſtillation of any ſubſtance, will be a tolerable meaſure of its fitneſs for affording light. In diſtillations of this kind, however, the degree of heat is of conſiderable moment, for I have found that the olefiant gas may be obtained or not, at pleaſure, during the decompoſition of ether, alcohol, oil, &c. by varying the temperature to which the containing veſſels are expoſed.

The gaſes from coal, &c. though they offered no olefiant gas, afford much light by an inflammable vapour when recent.

In the gaſes from coal, peat, and wood, though theſe ſubſtances yield no olefiant gas, the defect is compenſated by an inflammable vapour diffuſed through them when recent, and which is even not removed by paſſing through a ſmall quantity of water. Gas from coal, however, which had ſtood over water upwards of a month, I have found, burns with conſiderably impaired brilliancy, though ſtill with a far more denſe and bright flame than hydrogen gas, or the gas from charcoal.

Mancheſter, May 4, 1805.

POSTSCRIPT.

Since the preceding pages were written, I have examined a freſh portion of the gas from coal, obtained by very cautious diſtillation, with a view to aſcertain whether any olefiant gas can be procured from that ſubſtance. Of this gas five meaſures mixed with five of oxygenized muriatic acid, were reduced to nine; from which it ſhould appear that the gas from coal may poſſibly contain $\frac{1}{12}$ of olefiant gas. The production of oil, however, was not ſo manifeſt as in other inſtances; and I judged it to have happened chiefly becauſe an irideſcent film was viſible on the ſurface of the water when held between the eye and the light.

The gas from coal appears by exp. with ox. m. acid gas to contain a ſmall portion of ol. gas.

I am enabled alſo, by a letter received this morning from a friend who is well acquainted with the progreſs of Mr. Murdoch's experiments, in anſwer to ſome queries from me, to ſtate ſpecifically the grounds of that gentleman's claim to the important application of coal as a ſource of artificial light.—This I cannot do better than by an extract from the letter.

Hiſtory of Mr. Murdoch's experiments for giving light by gas from pit-coal.

"In the year 1792, at which time Mr. Murdoch reſided at Redruth in Cornwall, as Boulton and Watts principal agent and manager of engines in that county, he commenced a ſeries of experiments upon the quantity and quality of the gaſes contained in different ſubſtances. In the courſe of theſe, he remarked, that the gas obtained by diſtillation from coal, peat, wood, and other inflammable ſubſtances, burnt with great brilliancy upon being ſet fire to; and it occurred to him, that by confining and conducting it through tubes, it might be employed as an economical ſubſtitute for lamps and candles. The diſtillation was performed in iron retorts, and the gas conducted through tinned iron and copper tubes, to the diſtance of 70 feet. At this termination, as well as at intermediate points, the gas was ſet fire to, as it paſſed through apertures of different diameters and forms, purpoſely varied with a view of aſcertaining which would anſwer beſt. In ſome, the gas iſſued through a number of ſmall holes, like the head of a watering pan; in others it was thrown out in thin long ſheets, and again in others in circular ones, upon the principle of Argand's lamp. Bags of leather and of varniſhed ſilk, bladders, and veſſels of tinned iron were filled with the gas, which was ſet [illegible]o, and carried about from room to room, with a view of

aſcer-

Hiſtory of Mr. Murdoch's experiments for giving light by gas from pit coal.

aſcertaining how far it could be made to anſwer the purpoſe of a moveable or tranſferable light. Trials were likewiſe made of the different quantities and qualities of gas produced by coals of various deſcriptions, ſuch as the Swanſea, Haverford-weſt, Newcaſtle, Shropſhire, Staffordſhire, and ſome kinds of Scotch coals."

"Mr. Murdoch's conſtant occupations prevented his giving farther attention to the ſubject at that time; but he again availed himſelf of a moment of leiſure to repeat his experiments upon coal and peat, at Old Cumnock in Ayrſhire in 1797; and it may be proper to notice that both theſe, and the former ones, were exhibited to numerous ſpectators, who, if neceſſary, can atteſt them. In 1798, he conſtructed an apparatus at Soho Foundry, which was applied during many ſucceſſive nights, to the lighting of the building; when the experiments upon different apertures were repeated and extended upon a large ſcale. Various methods were alſo practiſed of waſhing and purifying the air, to get rid of the ſmoke and ſmell. Theſe experiments were continued, with occaſional interruptions, until the epoch of the peace in the ſpring of 1802, when the illumination of the Soho manufactory afforded an opportunity of making a public diſplay of the new lights; and they were made to conſtitute a principal feature in that exhibition. I do not know exactly at what time the firſt trials were made, or publiſhed in France. The firſt notice we received of them here, was in a letter from a friend at Paris, dated the 8th of Nov. 1801, in which he deſires me to inform Mr. Murdoch, that a perſon had lighted up his houſe and gardens with the gas obtained from wood and coal, and had it in contemplation to light up the city of Paris."

"After mentioning the above, I think it is proper to ſtate alſo, that in the ovens conſtructed upon Lord Dundonald's plan, at Calcutts in Shropſhire, for the purpoſe of ſaving the tar, &c. which eſcapes during the coaking of coal, it has been uſual for a number of years paſt to ſet fire to the large current of gas as it flies off, and thus procure a bright illumination. This however was not known to Mr. Murdoch, and was never ſeen by him."

II.

Experiments on the Analyſis of Goulard's Extract, or the Aqua Lithargyri acetati. By JOHN BOSTOCK, *M. D. Communicated by the Author.*

To Mr. NICHOLSON.

DEAR SIR.

I HAVE the pleaſure to tranſmit to you ſome experiments on the analyſis of Goulard's extract, which I hope you may think not unworthy of a place in your Journal.

I am, Sir,
Your obedient Servant,
JOHN BOSTOCK.

Liverpool,
May 5, 1805.

During the courſe of ſome experiments on the analyſis of animal fluids, I was led to obſerve the effect of the *aqua lithargyri acetati*, or the extract of Goulard, as a coagulator of mucus, and particularly to notice the ſuperiority of its power over that of the acetate of lead. From theſe circumſtances I was induced to examine into the opinions that had been entertained reſpecting its compoſition, but was not able to obtain any ſatisfactory information. Although it is a compound ſo well known, and ſo frequently employed, it appears never to have been made the ſubject of chemical analyſis. In Dr. Thomſom's Syſtem of Chemiſtry it is not diſtinguiſhed from the common acetate of lead;* Dr. Murray informs us, that "it is merely a ſolution of acetate of lead in water, with an "exceſs of acid;†" and Dr. Duncan, Jun. conceives, that it does not differ from a ſolution of the acetate of lead of the ſame ſtrength.‡ We meet with nothing ſpecific reſpecting its conſtitution in M. Fourcroy's "*Syſteme*", § nor is there any light thrown upon it by his acute commentator Prouſt.|| In this dearth of information, I proceeded to make the following experiments.

The extract of Goulard coagulates mucilage more than the acetate of lead.

It has not been examined.

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* Chem. III. 52. † Mat. Med. II. 223. ‡ Edin. Diſp. p. 506
§ VIII. 203. || Journ. Phyſ. LVI. 207.

To

Experiments on its compoſition. The common acetate of lead was diſſolved in water.

1. To 200 grains of diſtilled water were added 60 grains of acetate of lead, in its uſual cryſtalline ſtate; the fluid was kept for about an hour at the boiling temperature, and was afterwards filtered. The reſidue, when dried, did not amount to more than 2 grains; this was boiled in a freſh quantity of water, when about half of it was diſſolved, but one grain ſtill remained not acted upon by the water. It appeared, therefore, that a ſaturated ſolution of the acetate of lead was formed; it was tranſparent and colourleſs; it ſlightly reddened paper ſtained with litmus.

This was precipitated by carbonate of potaſh. It gave eleven parts carbonate of lead.

2. A ſolution of the carbonate of potaſh was prepared, in the proportion of 14.25 grains of potaſh to 100 grains of water. To 40 grains of the ſolution of the acetate of lead from No. 1, a quantity of this ſolution of potaſh was gradually added; a copious precipitate of the carbonate of lead was produced; after the addition of 60 grains of the alcaline ſolution there was no farther precipitation, and the fluid ſlightly affected a paper ſoaked in the infuſion of the mallow flower. The precipitate was carefully collected, and being dried by a gentle heat, afforded eight grains of carbonate of lead.

Goulard's extract gave eleven parts carbonate of lead.

3. Forty grains of *aqua lithargyri acetati* were treated in the ſame manner with the carbonate of potaſh; the precipitate formed appeared more copious than in the former experiment, and after the addition of 40 grains only of the alkali, no farther effect ſeemed to be produced; the fluid affected the mallow-paper in the ſame degree as in the former experiment. The precipitate, being collected and dried, weighed 11 grains.

It did not redden litmus paper. Blue precipitate with litmus infuſion.

4. The *aqua lithargyri acetati* did not in any degree redden litmus paper; a few drops of it were added to half an ounce of the infuſion of litmus; a precipitate of a beautiful light blue was immediately formed, while the fluid was left tranſparent and nearly colourleſs.

The ſolution of acetate gave 4 gr. brittle clear reſidue by evap.

5. Twenty grains of the ſolution of acetate of lead, No. 1, were ſlowly evaporated; the fluid became extremely viſcid, and at length, in ſome degree, brittle and tranſparent, and aſſumed the appearance of dried gum. It weighed about four grains.

Aqua lith. aceti gave 5 gr. white opake reſidue.

6. Twenty grains of the *aqua lithargyri acetati* were evaporated in the ſame manner; it became white and opake, and when the proceſs was completed, it exhibited the appearance of a number of ſcales of a pearl colour. It weighed a [illegible] more than five grains.

7. A

Gum precipitated by Goulard's extract.

7. A ſolution of gum Arabic was formed, in the proportion of one part of gum to 100 parts of water. One grain of this ſolution was added to 39 grains of water, ſo that the gum only conſtituted $\frac{1}{4000}$ part of the ſolution; a ſingle grain of Goulard dropped into it produced a perceptible opacity.

—but ſcarcely by acetate of lead.

8. Twenty grains of the ſaturated ſolution of the acetate of lead, had one grain of the ſolution of gum added; the effect was barely viſible, certainly leſs than in the former experiment.

I am far from conſidering theſe experiments as ſufficient to afford a complete inveſtigation of the ſubject; but I think they may enable us to make ſome advances towards the truth.

Forty grains acetate contain ſix grains metallic lead.

The 40 grains of the ſolution No. 1. contain 11.6 grains of acetate of lead; by the addition of the alcali, this was converted into eight grains of the carbonate of lead. Theſe eight grains of carbonate conſiſt of 6.72 grains of the yellow oxide, and 1.28 grains of carbonic acid.* The 6.72 grains of yellow oxide conſiſt of 6.12 grains of pure lead, and .6 grains of oxigen, † ſo that the 40 grains of the ſaturated ſolution contain a little more than 6 grains of pure lead.

Forty grains Goulard contain 8½ of lead.

By employing the ſame reaſoning to the analogous experiment with Goulard, we may conclude, that the 11 grains of carbonate produced in this caſe, conſiſt of 9.24 grains of the yellow oxide of lead, and 1.76 grains of carbonic acid; the 9.24 grains of oxide will be compoſed of 8.4 grains of pure lead, and .84 grains of oxigen, ſo that the 40 grains of the *aqua lithargyri acetati* contain nearly 8½ grains of pure lead.

Deduction of the reſpective quantities of acid.

We ſhall not find it ſo eaſy to aſcertain preciſely the quantity of acetic acid which enters into the compoſition of the acetate of lead, and the *aqua lithargyri acetati* reſpectively; but if we truſt to the experiments of M. Thenard‡, we muſt conclude that 11.6 grains of acetate of lead, contain about three grains of the acetic acid. The quantity of acid in the *aq. lith. acet.* is leſs than that in the acetate of lead, in the proportion of 40 to 60; therefore the 40 grains of *aq. lith. acet.* will only contain two grains of acid.

Component parts of each.

Hence it follows, that 40 grains of the ſolution of acetate of lead conſiſt of,

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* Thomſon's Chem. III. 50. † Prouſt, Journ. de Phyſ. [illegible]06. ‡ Nich. Journ. VI. 223.

Lead

	Grs.			Grs.
Lead - -	6.12	The ſame quantity of Goulard will conſiſt of - -	Lead	8.4
Acetic Acid	3.		Acid	2.
Oxygene	.6		Oxygene	.84
Water	30.28		Water	28.76
	40.00			40.00

converting theſe proportions into quantities of 100 grains each they will be as follows,

	Sol. acet. lead.	Aq. lith. acet.
Lead - - -	15.3	21.
Acetic acid -	7.5	5.
Oxygene -	1.5	2.1
Water - -	75.7	71.9
	100.0	100.0

The experiments ſhew that Goulard and the acetate are different ſalts.

From this ſtatement it appears, that in the *aq. lith. acet.* the oxide of lead and the acid exiſt to each other in the proportion of 23 to 5, or of 100 to 21.74, and we find that M. Thenard has deſcribed the perfect acetate of lead, as a ſalt in which the oxide and the acid exiſt in the proportion of 100 to 21.79. So near a coincidence between theſe two proportions can ſcarcely be regarded as the mere effect of accident; but muſt rather be conſidered as a proof that the ſubſtances operated upon were nearly, if not altogether, identical. Admitting this to be the caſe, we muſt conclude that the *aqua lithargyri acetati* is a ſaturated ſolution of the proper acetate of lead, that it is an eſſentially different ſalt from the ſuper-acetate of lead, and that it is not, as has been imagined, an accidental compound, but an exactly neutralized ſalt, the conſtituents of which exiſt in a conſtant ratio to each other.

The former being at ſaturation,

—and the latter a ſuper-acetate.

The neutral compound is moſt eaſily decompoſed,

—and is therefore a better teſt of mucus.

It happens in this, as in other inſtances, that the ingredients compoſing the completely ſaturated compound, [illegible]ſs a weaker affinity for each other than when they exiſt in a different proportion. To this circumſtance muſt be attributed the ſuperior delicacy which Goulard poſſeſſes, as a teſt* of animal and vegetable mucus, over the ſuper-acetate of lead, or the common *ceruſſa acetata.* The *aqua lithargyri acetati* is ſpeedily decompoſed by the action of the atmoſphere, in conſequence of the oxide of lead which enters into its compoſition having a ſtronger affinity for carbonic than for acetic acid; this effect takes place in a leſs degree, in a ſaturated ſolution of ſuper-acetate of lead.

From

Super-acetate of lead more ſoluble than generally ſuppoſed.

From the firſt experiment we learn, that the ſuper-acetate of lead is more ſoluble in water than is generally imagined; Dr. Thomſon obſerves, that it is diſſolved only ſparingly *; yet we find that 100 parts of water retain in ſolution 27 parts of the ſalt.

III.

A conciſe View of the Theory of Reſpiration. By W. BRANDE', *Eſq.* (*From the Author.*)

Reſpiration,

THE term reſpiration implies the reception of atmoſpheric air into the lungs, and its ſubſequent emiſſion, after having produced changes in the blood neceſſary to the continuance of life †.

could not be explained till the atmoſphere was analyſed.

No other gaſeous body being capable of producing theſe changes, it was natural to ſuppoſe, that until we became acquainted with the component parts of the atmoſphere, very little of the true nature of reſpiration could be underſtood.

Diſcoveries of Prieſtley, and Lavoiſier.

The firſt great ſtep towards the analyſis of the air was made by Dr. Prieſtley, who in the year 1774 diſcovered oxigen gas, called by him dephlogiſticated air. But we are indebted to Lavoiſier for the moſt accurate inveſtigation on this ſubject; who from many experiments, which it is not neceſſary here to relate, concluded that atmoſpheric air was compoſed of oxigen and azot, in the proportion of about 27 parts of the former to 73 of the latter. The air alſo contains a ſmall quantity of carbonic acid, and a conſiderable quantity of water (ſubject however to much variation) is always ſuſpended by it.

Gas which cannot be reſpired, or admitted into the lungs.

Some of the gaſes are totally unreſpirable, that is to ſay, incapable of being taken into the lungs; for whenever this is attempted, a ſpaſmodic affection of the epiglottis takes place, which by cloſing on the larynx, ſhuts up all communication with the organs of reſpiration. To this claſs belong all thoſe

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* Thomſon's Chemiſtry, III. 53.

† Reſpiration has been divided into, 1. Inſpiration, or the ingreſs of air into the cells of the lungs, cauſed by the enlargement of the cavity of the cheſt; 2. Into expiration, or the egreſs of air from the lungs, cauſed by the contraction of the cheſt.

gaſeous

Carbonic acid. Its effects; as deſcribed by Pilatre de Rozier.

gaſeous bodies poſſeſſed of acid properties. The effects of carbonic acid are deſcribed as follows by Pilatre de Rozier:— He went into a brewer's tub which was full of carbonic acid gas; he at firſt felt a ſlight heat throughout his whole body, which produced a gentle perſpiration; an itching ſenſation frequently obliged him to cloſe his eyes, and on attempting to breathe, he was prevented by a very violent ſenſe of ſuffocation. He wiſhed to get out of the tub, but being unable to find the ladder, the neceſſity of breathing increaſed, he was ſeized with a violent giddineſs, and felt a tingling ſenſation in his ears. He at length contrived to extricate himſelf, and although he then experienced no difficulty in breathing, he was unable to diſtinguiſh the objects around him; his hearing was alſo much impaired. On repeating the experiment he found, that as long as he remained without attempting to breathe, he could readily move or even ſpeak, but whenever he tried inſpiration, a violent ſenſe of ſuffocation came on.

Gaſes which can be admitted into the lungs.

Hydrogen and azot kill by mere ſuffocation.

Gaſes which kill by a ſpeedy poſitive action.

Nitrous oxide and oxigen kill by a ſlower action.

Atmoſpheric air maintains life.

But there are certain gaſeous bodies which may be drawn into the lungs, meeting with no oppoſition from the organs of reſpiration. Dr. Thomſon has divided theſe into four claſſes. The firſt ſet, he obſerves, occaſion immediate death, but produce no viſible change in the blood; they occaſion the animal's death, merely by depriving him of air, in the ſame manner as were he immerſed in water: the only gaſes belonging to this claſs are hidrogen and azot. The ſecond ſet occaſion immediate death alſo, but at the ſame time produce certain alterations in the blood; and therefore kill, not only by depriving the animal of air, but by certain ſpecific properties: the gaſes belonging to this claſs are, carburetted hydrogen, ſulphuretted hydrogen, carbonic oxide, and perhaps alſo nitrous gas. The third ſet of gaſes may be breathed for ſome time without injury, but death enſues at laſt, provided their action be long enough continued: to this claſs belong the nitrous oxide and oxigen gas. The fourth ſet may be breathed any length of time without injury: the only gaſeous body belonging to this claſs, is the air of the atmoſphere, that compound of oxigen and azot every where ſurrounding the globe.

This compound fluid loſes its oxigen by that proceſs, and carbonic acid gas is produced.

After an animal has breathed a certain quantity of air for a given time, it becomes totally unfit for reſpiration; and if the air thus reſpired be chemically examined, we ſhall find that the oxigen is greatly diminiſhed, and that a conſiderable tity of carbonic acid gas has been produced.

It appears from a number of experiments made by Dr. Hales, Dr. Menzies, and Mr. Davy, that the number of reſpirations made in a given time, as well as the quantity of air taken into the lungs, are liable to conſiderable variations in different people. Some have calculated the number of reſpirations at 14 only in a minute, others at 20; Mr. Davy informs us that he makes 26 or 27 in a minute; but having frequently endeavoured to count the reſpirations made by different people, in a given time, and without their knowledge, I have found them vary from 18 to 26 in a minute, moſt commonly, however, 20 or 21; and 21 in a minute make 30,240 in 24 hours.

The number of reſpirations made in a minute vary in different perſons from 18 to 26; but are commonly about 20.

The quantity of air taken in at each reſpiration, muſt be in proportion to the ſize of the perſon and the capacity of his lungs. About 41 cubic inches of air are taken in at every natural inſpiration.

Each reſpiration being about 41 cubic inches of air.

We now come to conſider the changes which are produced, both in the air and blood, by reſpiration. 1. On the changes effected in the air. Dr. Prieſtley, M. Lavoiſier, and Mr. Davy, have furniſhed us with many intereſting and inſtructive experiments on this ſubject. The changes are, 1. That a portion of the air diſappears; 2. That the air expired differs from that firſt taken into the lungs, in containing carbonic acid, and water in the ſtate of vapour. Dr. Menzies has ſhewn, that $\frac{1}{20}$th of the air inſpired diſappears in the lungs, and the experiments of Lavoiſier, which were made with much preciſion, differ but little from the above ſtatement. I never knew the quantity of air which diſappears to be leſs than $\frac{1}{19}$th part of the whole taken into the lungs; this may however be liable to variation in different people.

Phenomena of reſpiration.

Some air diſappears; about one-nineteenth; and the expired air is altered.

It has hitherto been ſuppoſed that the portion of air which diſappears, conſiſts of the oxigen only: Mr. Davy has, however, given ſome very ſtrong reaſons for ſuppoſing that part of the azote likewiſe diſappears during reſpiration. He ſuppoſes that the average quantity of air which is abſorbed at every reſpiration, amounts to 1.4 cubic inches, of which 0.2 are azot and 1.2 oxigen.

The abſorbed air was thought to be oxigen only: but appears to be in part azot.

Lime-water detects carbonic acid gas in the air emitted from the lungs, and the quantity of this gas may eaſily be eſtimated, by receiving the air expired into a graduated glaſs jar, ſtanding over mercury; a little cauſtic ſoda being introduced, the abſorption which takes place denotes the quantity of

The emitted air contains carbonic acid;

about one cubic inch each expiration.

But this varies.

of carbonic acid. Lavoiſier has eſtimated the quantity of this gas thrown out from the lungs in 24 hours, at about 15.5 ounces troy. Mr. Davy makes the quantity thrown out in the ſame time, amount to 37 ounces, which is about 1 cubic inch at every expiration *. The quantity however varies from 0.5 cubic inch to 1.5 at different times in the ſame perſon, ſo that this accounts for the great variation in the above-mentioned experiments. Moreover the proportion varies, in the ſame individual, during the 24 hours; for I have found the quantity of carbonic acid gas emitted from my own lungs, to be rather leſs in the morning than towards the evening; but this alſo varies in different people.

Aqueous vapour alſo emitted;

But watery vapour is alſo emitted in reſpiration, the greateſt part of which is probably given off by the exhalent arteries, which are ſo copiouſly diſperſed on the ſurface of the lungs. A part is alſo emitted from the blood in the pulmonary veſſels.

its quantity variable.

The eſtimation of its quantity is attended with ſome difficulty; according to Dr. Hales it amounts in a day to 20 ounces: this is however but of little conſequence, for it is liable to much alteration.

The blood undergoes important changes during reſpiration.

It abſorbs air; becomes florid red; emits carbonic acid; and water; and perhaps hydrogen.

Various theories.

More important changes however than thoſe juſt mentioned are produced by reſpiration, namely, the alterations produced in the blood; which fluid, returning from every part of the body by the veins, is poured into the heart; from whence, being propelled through the lungs, it is brought into contact with the air, undergoing certain changes which render it fit for the nouriſhment and ſupport of the body. Theſe changes, which are of a very complicated nature, have engaged the attention of ſeveral learned and ingenious philoſophers. Dr. Thomſon has enumerated them as follows: 1. The blood abſorbs air. 2. It acquires a florid red colour, and the chyle diſappears. 3. It emits carbonic acid, and perhaps carbon. 4. It emits water, and perhaps hydrogen. Dr. Prieſtley, M. Lavoiſier, and Lagrange, have each adopted a different theory, by which they endeavour to explain and account for theſe changes produced by reſpiration: they are all however liable to conſiderable objections, and reſt merely on the ſuppoſition that the oxigen is alone abſorbed. Now Mr. Davy has ſhewn, that at leaſt a portion of the azot diſappears in the

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* Davy's Reſearches, page 433.

lungs: he has even rendered it very probable that the air is not decompoſed, but that it is abſorbed unaltered by the blood; that it is decompoſed during circulation; and that the uſeleſs portion of azot is again given out. The following facts are in ſupport of this opinion: "When the gaſeous oxide of azot is reſpired, its quantity is diminiſhed, carbonic acid gas is evolved as uſual, and a quantity of azot makes its appearance. Now as this azot did not exiſt ſeparately, it muſt have been produced by the decompoſition of the gaſeous oxide of azot; but its quantity being much leſs than the azot contained in the oxide of azot which had diſappeared, it follows that a part of this laſt gas had been abſorbed unaltered; and if a part, why not the whole? In that caſe the azotic gas muſt have been ſeparated from the blood by the ſubſequent decompoſition of the oxide of azot abſorbed *." Atmoſpheric air is compoſed of exactly the ſame ingredients as the oxide of azot, merely in different proportions, and in a ſtate of leſs intimate chemical combination. It is moreover natural to aſk, that if oxigen were alone abſorbed by the blood, why ſhould it not anſwer the ſame purpoſes as air? It is well known that this gas cannot be reſpired for a length of time without producing fatal conſequences; but even when it is reſpired, the quantity (of oxigen) which diſappears is much ſmaller than when a like quantity of atmoſpheric air is breathed for the ſame time. Mr. Davy has given the following experiment in proof of this fact: He breathed 182 cubic inches of oxigen gas for half a minute, 11.4 c. inches diſappeared; whereas when the experiment was repeated under the ſame circumſtances with atmoſpheric air, the quantity abſorbed amounted to 15.6 cubic inches.

Mr. Davy apprehends that azot is abſorbed with the oxigen, &c.

Facts from the reſpiration of oxide of azot.

Oxigen alone is not proper for reſpiration;

Much leſs is abſorbed than when a like quantity of atmoſpheric air is breathed.

It was firſt obſerved by Lower, that the colour of venous blood, which is dark reddiſh purple, was converted into the florid ſcarlet colour of arterial blood, in its paſſage through the lungs. The phenomena of reſpiration, however, ſtill remained unexplained, until Dr. Prieſtley publiſhed his experiments on the changes produced in venous and arterial blood when put in contact with certain gaſeous bodies. "† He obſerves, that having introduced pieces of the craſſamentum of

Lower firſt obſerved the change of colour in venous blood by reſpiration.

Prieſtley ſhewed the cauſe; viz. the abſorption of oxigen.

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* Thomſon's Syſtem of Chemiſtry, Vol. IV. page 719.

† Prieſtley on Air, Vol. III. page 71.

coagulated ſheep's blood into dephlogiſticated air (oxigen gas); the blackeſt parts aſſumed a florid red colour, and that more readily than they would have done if common air only had been made uſe of: Whereas the brighteſt red blood became preſently black in any kind of air unfit for reſpiration, as in fixed air, *&c.*; and after having become black in phlogiſticated air (azot), it regained its red colour on being brought into contact with common air, the ſame blood becoming alternately black and ſcarlet, by being transferred from phlogiſticated into dephlogiſticated air, and *vice verſâ*."

Theſe then may be regarded as the experiments which gave origin to all ſubſequent enquiries.

Deſcription of the proceſs of nutrition.

Formation of fibrina.

The food which is taken into the body is converted into chyle and excrement [*]; the former of which is abſorbed by a ſet of veſſels termed lacteals, which convey their fluid into the thoracic duct. The term lymph has been applied to that fluid which lubricates the ſurfaces of all the circumſcribed cavities of the body: This fluid is abſorbed by a ſet of veſſels termed lymphatics, which of courſe originate in every part of the body; they likewiſe terminate in the thoracic duct, which therefore is the great reſervoir of the abſorbent ſyſtem; it receives the chyle and lymph, and conveys them to the blood; they are here decompoſed, and converted into new ſubſtances neceſſary to the ſupport of the body. Now the coagulable lymph, or fibrina, appears to be the moſt eſſential part of the blood, for it is employed to ſupply the waſte of the muſcles, *&c.* and Dr. Thomſon has accounted for its formation in the following mannner [†]: "It follows," ſays he, "from the experiments of Fourcroy, that fibrina contains more azot and leſs hydrogen and carbon than any of the ingredients of the blood, and conſequently alſo than any of the ingredients of the chyle. In what manner the chyle, or a part of it, is converted into fibrina, it is impoſſible to ſay: We are not ſufficiently acquainted with the ſubject to be able to explain the proceſs. But we can ſee at leaſt, that carbon and hydrogen muſt be abſtracted from that part of the chyle which is to be converted into fibrina, and we knew that theſe ſubſtances are actually thrown out in reſpiration. We may conclude then that *one* uſe of the air abſorbed is to abſtract a quantity of carbon and hydrogen from a part of the chyle by compound affinity, in ſuch a manner that the remainder becomes fibrina: Therefore one end of reſpiration is to form fibrina.

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[*] The food, on being received into the ſtomach, is converted into a pulpy ſubſtance termed chyme. This alteration is effected by a peculiar fluid called *gaſtric juice*, which is ſecreted by the internal coats of the ſtomach. The chyme thus formed is propelled into the duodenum, where it meets with the bile, which converts it into a fluid much reſembling milk, termed chyle, and into excrement.

[†] Thomſon's Chemiſtry, 2d Edit. Vol. IV. page 725.

Uſe of reſpiration in that connexion.

It appears then, from the above-mentioned facts, that the perfection of the blood is almoſt totally dependent on reſpiration; whenever therefore this function is ſuſpended but for a very ſhort time, death is the conſequence.

Life cannot ſubſiſt without it.

It is well known that all the more perfect animals poſſeſs a temperature conſiderably higher than the ſurrounding atmoſphere: the cauſe however of this increaſed temperature, remained unexplained for a conſiderable time. At length Dr. Black's theory of latent heat became known, when ſeveral attempts were made to explain the cauſe of the increaſe of temperature, or ſtandard heat of the body, but none of them were ſatisfactory. Dr. Thomſon has however given us the following ingenious theory: As the air is abſorbed unaltered by the blood, it is evident that it will give out the greateſt portion of its caloric during circulation; that portion therefore which is emitted at the inſtant that the air combines with the blood, is united to the carbonic acid, converting it into the ſtate of gas, and the water into vapour. It appears moreover, that the heat of the blood is ſomewhat raiſed during circulation; for Mr. John Hunter found that the blood in the heart was a degree higher than in any other part of the body.

The elevated temperature of the more perfect animals is cauſed by the combination and condenſation of air.

From the facts which have now been alluded to, it appears that the following changes are produced by reſpiration: The blood is propelled, by the contraction of the heart, into the pulmonary artery, which, by its numerous ramifications, conveys the blood into the ſmall branches of the air-cells of the lungs, which are of ſo fine a texture as to admit the abſorption of a portion of air. The blood having undergone this alteration, is returned into the heart by the pulmonary veins, from whence it is circulated over the whole body. During the circulation, the air which has been abſorbed undergoes a gradual decompoſition; carbonic acid and water are formed, which, together

Recapitulation.

together with a portion of azot, are returned by the veins, and thrown out as the blood paſſes through the lungs. A freſh portion of air is at the ſame time abſorbed, and the above changes repeated.

Theſe then are the effects of reſpiration, as far as we are at preſent acquainted with them; but this important branch of phyſiology ſtill remains in conſiderable obſcurity.

Arlington Street, WILLIAM BRANDE.
April 29, 1805.

IV.

Inſtruction on the Proceſſes diſcovered by M. BRALLE, *of Amiens, for watering Hemp in Two Hours Time, in all Seaſons, without injuring its Quality*[*]. *Publiſhed by Order of the Miniſter of the Interior of France.*

Experiments on hemp, by order of the French government.

IN the month of Fructidor in the year XI. (September 1803.) the government called to Paris M. Bralle, of Amiens, the inventor of new proceſſes for watering hemp. This diſcovery, which is intereſting to agriculture, manufactures, commerce, and the marine, had engaged its attention; orders were given to make the experiments requiſite to aſcertain its value.

Every thing which could elucidate the principles and practice of M. Bralle's proceſſes, which could prove and inſure their ſucceſs, was put in practice. Numerous and varied trials were made in the preſence of M. M. Monge and Berthollet, ſenators, and Teiſſier, member of the Inſtitute. M. Molard, adminiſtrator of the conſervatory of arts and manufactures, directed theſe trials, and carefully purſued them for ſix months. The reſults were equal to the expectations that had been formed.

From the account rendered to his imperial Majeſty, it was judged that the knowledge of a more expeditious method of watering hemp, than thoſe employed at preſent, which is practicable at all ſeaſons, and is in no reſpect injurious to health, by means of which a greater produce can be obtained from an equal quantity of the materials; and, which muſt extend and multiply the culture of an extremely valuable plant, could not be too extenſively publiſhed. In conformity to this deſire, we ſhall briefly deſcribe M. Bralle's proceſſes, relate the experiments which have been made, and offer ſome obſervations on the utility and advantages which are promiſed by this new diſcovery.

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[*] From Bibliotheque Phyſico Economique, Brumaire, An. XIII,

§ I.

The Proceſſes of M. Bralle.

The means uſed by M. Bralle for watering hemp, are

1ſt. Water is heated in a veſſel to the temperature of from 72° to 75° of Reaumur's thermometer; (200° fahr.)

2nd. A quantity of green ſoap (*ſavon verd*) is added proportional to the quantity of the hemp to be ſteeped.

M. Bralle's proceſs. The hemp is ſteeped in hot water with ſoap.

3rd. The hemp is then immerſed ſo that it ſhall be covered by the fluid, after which the veſſel is cloſed, and the fire put out.

4th. The hemp is left in this ſtate of maceration for two hours, and then taken out.

The weight of ſoap required for a complete ſteeping, is to that of hemp-ſtalks as 1 to 48; and the weight of the hemp to that of the water as 48 to 650.

Proportions of the articles.

Several ſteepings may be made one after the other. It is ſufficient, before each new immerſion, to add a quantity of ſoap water to replace what was abſorbed by the preceding, and to raiſe the temperature of the bath to the above degree. The ſame water may be thus employed for fifteen ſucceſſive days.

The proceſs may be repeated with other hemp in the ſame water.

When the bundles are taken out of the ſteeping veſſel, they are covered with a layer of ſtraw, that they may cool gradually, without loſing their humidity.

Next day they are ſpread on a floor, puſhing the bands towards the top of the ſtems, and a roller of ſtone or wood, loaded with a weight, is paſſed ſeveral times over them, to cruſh them, and diſpoſe the tow to be eaſily ſeparated from the reed, which is effected by beating. Whether the hemp be wet or dry, it peels completely in either ſtate.

Breaking the ſtalk, beating.

After having tied the handfulls of the tow peeled off while wet, at the top, they are ſpread on the graſs, turned, and, after five or ſix days, carried to the warehouſe.

The handfulls of ſteeped and cruſhed hemp which are intended to be beaten and ſtripped dry muſt alſo be expoſed on the graſs; this operation being abſolutely neceſſary to whiten the tow, and facilitate the ſeparation of the reed.

§ II.

Recapitulation of the Experiments.

The experiments were varied to aſcertain the requiſite

By means of a portable ſteeping veſſel, different quantities of hemp were ſteeped, the temperature of the ſoapy liquor was varied at pleaſure, and the ſtate of the hemp was obſerved during the courſe of each operation, of which the duration was more or leſs prolonged, in order to aſcertain;

Temperature,

1ſt. The temperature which the ſoapy liquor ought to have before the immerſion of the hemp;

time, and

2nd. The time neceſſary for a complete ſteeping, at a determinate temperature;

proportion of ſoap.

3rd. The quantity of ſoap abſolutely neceſſary for a given weight of hemp-ſtalks, weighed before the immerſion, &c.

Reſults or general obſervations on the proceſs.

From a great number of experiments made in the months of January, February, and March laſt, it was found,

1ſt. That water containing the quantity of green ſoap directed by M. Bralle, for a given weight of hemp, effects the ſteeping completely;

2nd. That the ſteeping is ſo much the more ſpeedy as the temperature of the fluid is nearer to ebullition, at the time of the immerſion of the hemp;

3rd. That if the hemp be kept more than two hours in the ſteeping veſſel, the time preſcribed by M. Bralle for obtaining a complete watering, the tow ſeparates equally well from the reed, but it acquires a deeper colour, and loſes part of its ſtrength;

4th. That if the hemp be immerſed in a cold ſoapy liquor, and heat be then applied, the ſteeping is not accompliſhed ſo perfectly, whatever degree of temperature may be given to the liquor, and however long the immerſion may be continued *;

5th. That the bundles of hemp immerſed and kept vertically in the veſſel, are ſteeped more uniformly than if they were laid horizontally; and this poſition alſo facilitates the manipulation.

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* Probably the fluid between the fibres is not heated, becauſe its conducting power is bad, and it is prevented from circulating. N.

§ III.

Obſervations on the Utility and Advantages of the New Diſcovery.

Deſcription of the methods of ſteeping hemp, as heretofore practiſed. Expoſure on the graſs.

Two methods only of ſteeping hemp are generally practiſed. The firſt conſiſts in ſpreading the plant on the graſs, and turning it two or three times a week, until the air, the light, the dews, or the rains, have diſpoſed the tow to ſeparate eaſily from the reed. The reſult is obtained in a longer or ſhorter time, according to the weather, and the ſtate of the air; and frequently, in certain countries, the operation is not finiſhed in leſs than forty days.

2. Steeping.

The ſecond conſiſts in immerſing the bundles of hemp in rivers, brooks, ditches, or pools, and keeping them there for eight, fifteen, twenty, or even thirty days, according to the degree of the heat of the water, or of the atmoſphere.

Theſe proceſſes very defective.

The maceration effected by both theſe proceſſes is frequently incomplete, and always unequal. By following the firſt, the cultivator is liable to have his crop diſperſed by the winds, or injured by long rains: if he adopts the ſecond, he runs the riſk of loſing a part of it by the overflowing of the rivers, or of its being covered with mud. The firſt method in particular, is liable to the ſerious inconvenience of depriving the national marine of part of the hemp produced by our territory: it is known that the tow produced from the hemp which has been expoſed on the graſs is not uſed by the government.

Superiority of the new proceſs.

The ſteeping of hemp according to M. Bralle's proceſs, requires only a copper cylindrical veſſel, placed on a ſmall furnace of bricks.

A ſteeping veſſel of this kind, containing 240 litres of water, (52 ale gallons) is ſufficient to ſteep 18 kilogrammes of hemp-ſtalks, (about 40lb.) and as the operation is completed in two hours, 100 kilogrammes (221lb.) may be eaſily ſteeped in a day.

This method appears to deſerve the preference over the former ones, on many accounts.

It is practicable all the year.

1ſt. The ſteeping is practicable all the year, except during very hard froſts, when it is difficult to dry the hemp. But when it is to be peeled wet, the cold is no longer an obſtacle; it is then only neceſſary to take proper precautions for preventing the tow from freezing in its humid ſtate.

ſaves time,

2nd. The time of ſteeping being only two hours, affords a ſaving of time of great value to the cultivator, particularly during the ſeaſon of harveſt.

and is not injurious to health.

3rd. The workman has no cauſe to fear any injury of his health: it is ſufficient to keep up a current of air while the bundles are plunged into and taken out of the ſteeping veſſel; the handfulls of ſtalks or tow, which are afterwards expoſed on the graſs, do not emit any bad ſmell, or vitiate the purity of the air, whatever may be the quantity of hemp dried at once in the ſame place.

Every one knows, that when the bundles of hemp ſteeped in water in the old method, are taken out and waſhed, they emit an infectious odour which becomes inſupportable during the heats, and to which ſerious diſorders are aſcribed. The valley of the department of the Somme, and many others in which hemp is ſteeped, afford too convincing proofs. The waters are rendered unfit for the uſe of cattle, and the fiſh contained in them are frequently deſtroyed.

Apparatus on a larger ſcale.

To accelerate the operation of ſteeping by the new proceſs, in countries where there is an extenſive culture of it, inſtead of the portable ſteeping veſſel which was made uſe of in the experiments, the following apparatus may be adopted, conſiſting of a boiler and four wooden tubs, ſerving for ſteeping veſſels.

After having heated the ſoapy matter to ebullition, it is ſuffered to flow through a cock, into two of theſe tubs filled with bundles of hemp, and cloſed by a cover: while the ſteeping is going on in the two firſt tubs, the neceſſary quantity of liquor is heated, to be conveyed into the other two, which are alſo filled with bundles of hemp, and cloſed with lids.

By means of this very ſimple apparatus, a conſiderable quantity of hemp may be ſteeped in a day without interruption.

Comparative expence of the two proceſſes.

4th. The expence of ſteeping in water, compared with that required by the method of M. Bralle, is nearly the ſame, when the ſmall ſteeping veſſel is made uſe of; but if a cauldron, rather large, and the ſteeping tubs which have been mentioned are employed, the coſt will be diminiſhed more than a half.

In fact, the expence of the firſt includes the conveyance of the hemp to be ſteeped, the time employed in forming the bundles of hemp into a ſort of rafts, that they may be ſunk by loading them with ſtones, turf, clods of earth, and even mud; in fixing and ſecuring theſe rafts by driving in ſtakes; a tedious work, and the more troubleſome, becauſe 10 kilogrammes of hemp-ſtalks cannot be immerſed without a weight of 15 or 20 kilogrammes, and, after the ſteeping, all this maſs muſt be removed, to take the bundles out of the water, and waſh them.

The coſt of the new proceſs conſiſts principally in the price of the ſolvent made uſe of, which amounts to about eight centimes for a kilogramme of tow. To this ſhould be added the price of combuſtible neceſſary for heating the liquor, if this combuſtible was not afforded by the reeds of the bundles, whether they are peeled wet or dry.

At an equal expence, the new proceſs is ſtill preferable to the old, becauſe, from what has been ſaid, it renders the manipulation more expeditious and more eaſy.

Comparative produce.

5th. Eight kilogrammes of hemp-ſtalks ſteeped by the new proceſs, commonly produce two kilogrammes of pure tow, by peeling when wet; whereas hemp ſteeped in water by the old proceſs, and beaten, does not yield more from eight kilogrammes than one and a half.

The dry peeling of hemp ſteeped in the old way does not produce the ſame quantity as that which is peeled when wet: the breaking of the reed in many places occaſions a greater loſs of tow.

The hemp being waſhed, beaten and combed in the old method, a kilogramme of long tow is obtained from four kilogrammes of the rough tow; the remainder is ſhort ſtuff, hards and duſt.

The ſame quantity of hemp, manipulated in the new way, yields two kilogrammes of long tow, one kilogramme of ſecond tow, and about a kilogramme of ſhort ſtuff and hards.

Thus from eight kilogrammes of hemp-ſtalks, two kilogrammes are obtained in rough tow by the new proceſs, and from this quantity is obtained one kilogramme of the firſt tow, which does not exiſt in any known manipulation.

Extenſion of the culture of hemp.

6th. The inhabitants of the banks of rivers and of the valleys, are almoſt the only perſons who cultivate hemp: they owe this privilege to the vicinity of the waters, and the humidity of the ſoil. By the new proceſs the culture of hemp will be extended to all places, and procure a new and very advantageous occupation to the inhabitants of the plains, the land of which is much more vegetative than that of the marſhes.

It is an error to ſuppoſe that hemp cannot grow to a great height in the plains; it is a fact, that it riſes to the height of two yards, in land which has been well ploughed and manured, when mild rains have promoted germinanation and growth.

It is equally a fact, that there is every where a ſufficient quantity of ſpring or ciſtern water to ſteep the hemp by the new proceſs: if droughts ſhould ſupervene, which beſides are only accidental, the ſteeping may be deferred.

It will, therefore, be poſſible to cultivate hemp in the plains, and in low lands, which are always rich and fertile, though frequently without ſprings of water, and to augment not only the maſs of our products, but alſo our riches of this deſcription, ſince one acre of good hemp yields as much profit as two acres of wheat.

Summary of the advantages ariſing from this proceſs.

Such are the effects which may be expected from M. Bralle's new method of ſteeping hemp. It is, as was obſerved at the commencement of this inſtruction, more expeditious than thoſe hitherto employed; it perfectly completes the ſteeping; it may be uſed at all ſeaſons; it does not affect the purity of the air; from an equal quantity of materials, it procures a more abundant produce; and laſtly, it is well calculated to extend the cultivation of the plant itſelf. The enlightened lovers of agriculture, and well-informed proprietors, who live upon and cultivate their own eſtates, without being ſlaves to the cuſtomary practices, will adopt it, and ſecure its advantages, by repeating the experiments which have aſcertained its merit, and alſo by making trials on a more extenſive ſcale than thoſe which took place in the conſervatory of arts and manufactures. Their example will be followed, the proceſs of M. Bralle will be extended, and we ſhall ſee portable ſteeping veſſels, ſimilar to thoſe uſed by M. Molard, multiplied; a cheap apparatus which requires very little repairs, and by means of which the hemp grown through the extent of one or of ſeveral communes may be ſteeped even in the field on which it grew.

V.

Deſcription of a Portable Steam Engine. By Mr. MATTHEW MURRAY.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of handing you the deſcription of a portable ſteam engine of my conſtruction, which you will have the goodneſs to inſert in your Philoſophical Journal. I will juſt obſerve it is reduced to the feweſt parts that practical utility will admit, which muſt neceſſarily render it of great advantage; as the ſimplicity of its parts make it nearly impoſſible to be out of order with a very moderate degree of management. The following deſcription and reference to the plate will explain the nature of this engine.

I am, Sir,
Your much obliged humble ſervant,
MATTHEW MURRAY.

Leeds, May 7th, 1805.

Deſcription of a Portable Steam Engine.——Plate VII.

Deſcription of a portable ſteam engine.

A A Repreſents the ground or floor on which the engine ſtands.

B Section of a receſs made in the ground for the beam O to work in.

C. Iron ciſtern reſting upon the ground or floor covering the receſs for the beam.

D An opening in the floor to admit a boy to oil the centers of the beam.

E A double ſteam cylinder, having an upright pipe in the intermediate ſpace, which effects a communication between the top and bottom and the valve box G.

F A ſteam pipe that communicates with the boiler through which all the ſteam paſſes and ſurrounds the inner cylinder in its way to the valve box, prior to its application againſt the piſton.

G The valve box fixed upon a projection from the cylinder bottom, having an opening or connection with the interval between the two cylinders. In this opening is a regulating valve

Deſcription of a portable ſteam engine.

valve for adjuſting the quantity of ſteam (that acts againſt the piſton) in its paſſage through the valve box. There are alſo three other openings in the bottom of this valve box, one of which connects with the top of the cylinder by the pipe in the intermediate ſpace, the ſecond with the bottom, and the third with the eduction pipe that leads to the condenſor. Two of theſe openings are alternately connected together by a ſlide valve,* while the third is left open for the admiſſion of ſteam to the piſton, this valve changes its poſition at the end of each ſtroke of the piſton, and performs all the purpoſes of the moſt complicated machine.

H The air-pump connected with a condenſer at the bottom of the eduction pipe.

I. The fly wheel fixed upon an axis which receives its motion from a crank connected with the beam by the rod K.

L L Two rods for connecting the motion of the piſton to the beam, theſe rods move perpendicularly by a motion which could not be conveniently ſhewn in this view without rendering it confuſed.

M A ſpherical triangle turn'd by the crank for moving the ſlide valve by the horizontal rod N that connects them together. This motion has the advantage of preventing the engine from ever turning the contrary way round from that which it is wanted to go, and prevents the noiſe that is uſually heard in engines.

O The beam attached to the bottom of the ciſtern C by means of the hanging carriages P.

Q A reſt or fixture in a wall for the end of the fly wheel ſhaft; this will vary according to the ſituation where the engine is to be fixed, or it may be ſupported by a metal ſtandard.

R. Index to the injection cock that admits water to the condenſer. *Note,* The ciſtern is to be kept nearly full of water during the time the engine is at work.

The cylinder G and valve box E muſt be ſurrounded on all ſides by a caſe (not ſhewn in this view) the ſpace between filled with charcoal to prevent the tranſmiſſion of heat, which if effectually done will work with the leaſt poſſible quantity of coals, as it combines the advantages of every other engine hitherto known. By detaching the air-pump and condenſer

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* For theſe valves I took out a patent in 1802.

(which

Deſcription of a portable ſteam engine.

(which may be done in half an hour) and where water cannot be had for condenſation, this engine may be worked by the preſſure of ſtrong ſteam alone, as the internal cylinder is kept as hot as the ſteam in the boiler.[*] This dangerous plan never ought to be reſorted to but in caſes of neceſſity, as it is no ſaving of coals, and as there can be no certain rule when to diſcontinue the uſe of the boiler, the weakneſs of which is not prevented by putting the fire in a tube in the inſide of it. This engine requires no framing nor mill-wright work in the fixing, but merely bolting down to the floor it ſtands upon. It takes up very little room, and all its parts are within reach, without the neceſſity of upper floors or ſtages, which would be the caſe if the beam was above; but by being fixed below and alone, it has no tendency to move from its ſituation.

VI.

Letter from Mr. J. C. HORNBLOWER, *Engineer, on the Meaſure of Force by Horſe Powers.*

To Mr. NICHOLSON.

DEAR SIR,

Uncertainty of what is meant by the power of a horſe.

I AM induced to trouble you on account of the preſent unſettled ſtate of things reſpecting what is uſually called *the power of a horſe*. I do not know why a matter of this ſort ſhould remain ſo diſregarded, eſpecially as it has ſo long become one of our data, comprehending the unities of weight, ſpace and time, by which we are to be underſtood in our communications on the ſubject, and by which we are to aſcertain the preciſe value or effect of any mill or engine in and about London. Indeed I do not know why it was adopted for the purpoſes intended, it being ſo indefinite.

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[*] Many engines are at preſent worked in London and elſewhere by the mere force of ſteam, without condenſation, under Trevithick's patent. The force is from 45 to 60 lb. on the round inch; a preſſure equal to about 25 fathoms of water at the moſt. Various aſſertions and reports concerning the ſafety, the economy, and the other effects of theſe engines have paſſed under my notice; but the intereſted ſituation of ſome of the narrators on both ſides, and the ſhort time of trial, have induced me to wait for more facts before I ſhould give any account of the engine in this Journal. I hope to do this a few months hence.—W. N.

This unity probably aroſe from ſteam engines being ſubſtituted for horſes.

I can eaſily conceive it probable that ſomebody who has employed horſes for ſome time in mill-work, may have applied to an engineer, and ſaid "I have a mind to have my work done by a ſteam engine inſtead of horſes, for I am to a point that I ſhall ſave money by it, and pleaſe to give me an eſtimate of the coſt of an engine that will do the work of my horſes;" and then the engineer ſets about getting information as to what may be deemed the effect a horſe can produce, and calls it the *horſe power;* and perhaps having Deſagulier's Experimental Philoſophy at hand, applies to him, and there he finds that a horſe will raiſe a hogſhead of water 50 feet high in a minute; then what is the weight of a hogſhead of water, and he finds from ſome particulars related by him, 2nd vol. page 505, that a hogſhead of water is equal to 550 lb. but of what meaſure is uncertain, for the ale hogſhead, 51 gallons, is 540 lb. and the wine hogſhead, 63 gallons, is 501 lb. reckoning the cubic foot at 1000 ounces avoirdupois.

Deſaguliers conſiders it as 550 lb. raiſed 50 feet per minute.

Another eſtimate; nearly double that of Deſaguliers.

Some engineers who have very unceremoniouſly taken the lead in this affair, have adopted I do not know what for a *datum*, but the reſult is this: An engine by calculating 10 lb. on the ſquare inch, making the whole preſſure = 1000, moving through 200 feet per minute, is called a *four-horſe engine*.—Let us ſee then what will be the effect of one horſe according to this fact.

$1000^{\text{lb.}} \times 200^{\text{ft.}} = 200000 =$ the whole effect, then $\frac{200000}{4}$ = the effect of one horſe. Now compare this with the eſtimate of Dr. Deſaguliers, which is a hogſhead of water at 550 lb. 50 feet high in a minute; $550 \text{ lb.} \times 50 \text{ ft.} = 27500$ and ; $50000 - 27500 = 22500 =$ the difference between one of Mr. Watt's horſes and one of the doctor's, on the former of which I make no comment.

Smeaton's eſtimate one-fifth leſs than Deſaguliers.

Mr. Smeaton, whom I hold as having ſuperior claim to precedency on ſubjects of this nature, has utterly diſapproved of Deſagulier's experiment by the moſt powerful conviction of its fallibility, formed by concluſions drawn from ſterling experience in the accompliſhment of works on a large ſcale; and he ſtates the greateſt effect to be 40 feet high in a minute; but as this is ſtill in the commonly received opinion as to the weight of the hogſhead, I would rather turn to those who have made their experiments on a weight of ſolid matter, expreſſed in terms which cannot be miſtaken.

I remember to have had ſome converſation on this ſubject many years ago, with the late Samuel More, at that time ſecretary to the Society for the Encouragement of Arts, &c. when he ſhewed an inſtrument conſtructed on purpoſe to determine the reſiſtance againſt horſes at plough. I do not recollect that I made any minutes on the reſult of our converſation, ſo can only ſay that his relation of the fact ſurpriſed me, until I came to compare it with the effect of horſes actually applied to overcome a load drawn up a ſhaft in a mine; but I had not the ſame means of determining the re-action that he had; however, the reſult of his experiment may be ſeen in the Tranſactions of that Society; ſome obſervations on it may be ſeen in the 3d vol. quarto, of the Philoſophical Journal, page 136, only there ſeems to be a miſtake in the deduction in the note: it ſhould be 264 : 10 : : 1375 : 52 +

Mention of experiments on the reaction againſt horſes, by the late Sam. More, Eſq. with an inſtrument. *It was a ſpring with graduation.*

I much wiſh to have an experiment like Mr. More's made by a ſledge drawn forwards and backwards on a level road, with Mr. More's inſtrument placed between it and the horſes; ſuch an experiment would be very practicable, and the ſmall deviation from the true level of a road would be compenſated by alternately going firſt one way and then the other.

Propoſed experiment.

It is true that we are become pretty well acquainted with what may be done by horſes in grinding malt, pumping liquor and worts in breweries; but there are ſo many fortuitous circumſtances to be regarded, even here, that nothing decided can reſult from the cloſeſt inveſtigation. For inſtance, ſome brewers chuſe to have their malt ground much lower than others; the pump-work is executed in ſome breweries under very different advantages, and from local circumſtances may be retarded by the inertia neceſſary to communicate motion from the wheel to the work, adding the different condition of valves, buckets, &c. All theſe conſiderations demand ſome *invariable* reſiſtance to be overcome by the exertion of the horſe, and I know of nothing ſo appropriate as that I have juſt mentioned.

Uncertainty of the meaſure of a horſe power from grinding or pumping, &c.

The power of a horſe (by which I mean the mechanic power) is not eaſily aſcertained. It has ſcarce any analogy

Difficulty of this ſubject, not only from the work, but the claſs of horſes employed.

with a weight deſcending through a given ſpace or a quantity of water falling a given height, and therefore is better expreſſed by the terms effect, reſiſtance, re-action, &c. and even then, to be any thing like preciſe, we ought to diſcriminate whether brewers' horſes, or higlers' horſes, waggon horſes or coach horſes, heavy horſes or light, and if you will go into the country among the coal-mines, you will have another claſs of theſe animals, which I know not what to call unleſs it be *poor horſes*, full worked and half ſtarved; in ſhort, I mean that neither one or the other ought to be taken into the account as the *meaſure of a mechanic power*.

Mr. More's eſtimate of 80lb. 3 miles an hour. This is nearly three fourths of Deſagulier's rate.

Nevertheleſs it ſeems deſirable to have ſome popular expreſſion for the application of whatever may be ſubſtituted in the place of horſes, whether ſteam, water or wind; nor can there be any objection to ſaying, "equal to the work of ſo many horſes," provided we can attain to a clear, unequivocal and ſomewhat exact value, attributable to that power, and if I may give my own opinion, I think Mr. More has ſtated the utmoſt effect to be 80 lb. three miles per hour, in ſuch horſes as are proper for giving motion to mill-work, and at ſuch ſpells as will not exhauſt the breath or ſtrength of the animal.

Remarks.

I am ſurpriſed to find this mode of calculation has obtained ſo far as to determine the power engines employed purely as pumping engines, as lately at the Tunnel, the New Docks, &c. but I am glad it reaches no further than the bills of mortality, and I hear that the Dutch method of hoiſting goods to warehouſes has lately been adopted at ſome of our new docks. *O tempora, O mores!* While other countries are availing themſelves of the application of the ſteam engine in place of animal labour, we are taking up the expedients of thoſe who have ſcarce heard there is any ſuch thing as a ſteam engine, or who cannot appreciate its value on that degree of evidence we have in our own country.

Whether the horſe power or unity be true or not, it ought ſurely to be free from ambiguity.

It may be objected to by ſome to alter the preſent data, however erroneous, as we ſhall be obliged to require 20 horſe engines inſtead of 10 (for it appears the eſtimate is nearly, if not quite *cent. per cent.* more than it ſhould be); but even that can make no difference in any reſpect than as making a rent in an egregious error; the coſt of an engine cannot be altered by it, nor the conſumption of fuel, but a material convenience would be the reſult of ſuch a regulation, conſidering the advantage of a coincidence in this point throughout the kingdom, and as partaking of the nature of a unity of weights and meaſures, it ought to be paramount to every ſubordinate conſideration.*

Your much obliged obedient ſervant,

J. C. HORNBLOWER.

VII.

Letter from Mr. A. F. THOELDEN, *communicating three manuſcript Tables from Mr.* BODE, *of Berlin, of the geocentric Places of the new Planets Ceres, Pallas, and Juno, for twelve Months to come.*

To Mr. NICHOLSON.

SIR,

Tables obtained from Mr. Bode of the places of the new planets.

THE three planets, or aſteroids (according to Dr. Herſchel), lately diſcovered, being ſo very ſmall, are not eaſily found, unleſs the obſerver is acquainted with the place where he is to look for them. This uncertainty induced ſome aſtronomical gentlemen to deſire me to inquire, if there were not any ephemeris of their motions publiſhed abroad. I complied with their requeſt, and Mr. Bode has very obligingly communicated to me the following written account of their reſpective ſituations, calculated for the Obſervatory at Berlin, (that of Juno, according to a table of his own calculation). If you think this communication may be intereſting to the aſtronomical readers of your Philoſophical Journal, I beg you will make uſe of it.

I am, Sir,

Your moſt obedient humble ſervant,

A. F. THOELDEN.

10, *St. Alban's Street,*
May 13, 1805.

P. S. A new edition of Mr. Bode's ſmall Celeſtial Atlas has juſt been publiſhed; with a Catalogue of 5500 Stars, after Piazzi's obſervations. This laſt work can be had ſeparate.

Any gentleman who may be deſirous of one or both theſe works, will be ſupplied in a reaſonable time after ſending an order to me..

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* For a very clear and rational report of a ſteam-engine in horſe-powers, ſee our Journal, IX. p. 215.—W. N.

TABLE I.

GEOCENTRIC MOTION OF CERES.

1805.	A. R.	Decl.	1806.	A. R.	Decl.
Oct. 2.	104° 34′	22° 51′N.	Jan. 9.	103° 26′	29° 46′N.
11.	106 53	23 3	18.	101 11	30 25
20.	108 49	23 18	27.	99 18	30 53
29.	110 21	23 39	Feb. 5.	97 56	31 11
Nov. 7.	111 24	24 5	14.	97 13	31 22
16.	111 54	24 40	23.	97 10	31 26
25.	111 49	25 22	Mar. 4.	97 46	31 25
Dec. 4.	111 7	26 12	13.	98 58	31 20
13.	109 48	27 7	22.	100 43	31 11
22.	107 59	28 3	31.	102 57	30 58
31.	105 46	28 58	Apr. 9.	105 32	30 40
			18.	108 28	30 18
			27.	111 40	29 51
			May 6.	115 6	29 19
			15.	118 42	28 40
			24.	122 27	27 56

TABLE II.

GEOCENTRIC MOTION OF PALLAS.

1805.	A. R.		Decl.		1806.	A. R.		Decl.	
Aug. 3.	59°	29′	3°	4′ S.	Jan. 3.	68°	34′	31°	16′ S.
12.	62	47	4	22	12.	67	51	29	27
21.	65	54	5	56	21.	67	48	27	12
30.	68	52	7	47	30.	68	24	24	39
Sep. 8.	71	34	9	53	Feb. 8.	69	37	21	54
17.	73	58	12	15	17.	71	26	19	3
26.	75	59	14	50	26.	73	45	16	10
Oct. 5.	77	35	17	37	Mar. 7.	76	33	13	20
14.	78	40	20	30	16.	79	44	10	35
23.	79	12	23	23	25.	83	16	7	59
Nov. 1.	79	8	26	8	Apr. 3.	87	6	5	31
10.	78	26	28	38	12.	91	11	3	21
19.	77	11	30	43	21.	95	28	1	22
28.	75	29	32	13	30.	99	57	0	19 N.
Dec. 7.	73	33	33	3					
16.	71	35	33	9					
25.	69	51	32	32					

TABLE III.

GEOCENTRIC MOTION OF JUNO.

1805.	Longitude.	Latitude.	1806.	Longitude.	Latitude.
Oct. 1.	4^s 28° 22′	6° 21′ S.	Jan. 1.	5^s 26° 39′	4° 18′ S.
11.	5 2 28	6 12	11.	5 27 25	3 56
21.	5 6 18	6 1	21.	5 27 29	3 31
Nov. 1.	5 10 23	5 47	Feb. 1.	5 26 45	3 0
11.	5 13 54	5 33	11.	5 25 19	2 22
21.	5 16 44	5 21	21.	5 23 20	1 44
Dec. 1.	5 19 23	5 8	Mar. 1.	5 21 33	1 15
11.	5 21 34	4 54	11.	5 19 0	0 33
21.	5 23 27	4 38	21.	5 16 41	0 10N.
			Apr. 1.	5 14 27	0 50
			11.	5 12 56	1 26
			21.	5 12 5	1 57
			May 1.	5 11 46	2 19

Letter

VIII.

Letter from Profeſſor Pini, *Inſpector of Mines to the Italian Republic, to* J. C. Delametherie, *on Corindon found in Italy.**

Beautiful red adamantine ſpar found in Italy.

THE intereſt you take in publiſhing diſcoveries in natural hiſtory in your excellent Journal, induces me to communicate to you a mineralogical rarity lately found on a mountain of the Italian republic: it is a very fine corindon, or adamantine ſpar, of a deep ruby colour. I ſaw it for the firſt time among the minerals which the learned Brochi, profeſſor of natural hiſtory at Breſcia, had made a ſhort time before in the department of Serio. At the firſt view he conſidered it as a feld-ſpar, of which it has all the appearance; and, in fact, the corindon being a ſubſtance which hitherto has only been furniſhed by countries far diſtant from us, it would have been imprudent to have judged otherwiſe at firſt: but the colour of ſtone, exactly reſembling that of a red corindon which I brought from Paris, given to me as coming from Madras, led me to ſuppoſe that it did not differ from it,

But as Profeſſor Brochi purpoſed meeting me in a ſhort time at Milan, to which place I was going, we poſtponed the verification of this ſuſpicion. When he ſaw the red corindon from Madras, in my poſſeſſion, he no longer doubted the identity of its ſpecies with that of our ſample. I afterwards diſcovered the ſame identity in the trials to which I ſubmitted it: the following are the reſults:

Examination. 1. Hardneſs. 2. Infuſibility. 3. Laminar texture. 4. Fiſſure and angle. 5. Reflection of light. 6. Sp. gravity.

1ſt. The corindon of Italy ſcratches the hardeſt rock-cryſtal. 2d. It does not melt before the blow-pipe, either alone or with the addition of borax. 3. Its texture is in laminæ, which follow different directions. 4th. Its fiſſure is triple, and when it is cut in the three directions, it offers a rhomboid, the acute angle of which is 64⅓°. 5th. Its croſs fracture ſhews the ſplendor of the diamond, and reflects the light, the flaſhes of which are almoſt the colour of ſilver, 6th. Its ſpecific gravity is 3.87, which is the mean of that of the true corindon.

Is found on a mountain of micaceous ſchiſtus.

Hitherto it has been met with in a mountain of micaceous ſchiſtus, in pieces of ſeveral inches in length, which are amorphous and opake, but ſemi-tranſparent on the thin edges, Profeſſor Brochi and I purpoſe making new reſearches there, which may lead to ſome more intereſting diſcovery.

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* From Journal de Phyſique, Vendemiaire, An XIII.

Countries where corindon is found.

In the mean time it will no longer be doubted, that the corindon is a product of Europe. M. de Bournon, with whoſe memoir on corindon, inſerted in the Journal des Mines, Vol. XIV. you are well acquainted, has noticed the different countries which furniſh this ſubſtance; they are the iſland of Ceylon, the peninſula of India, and in particular Madras, the Carnatic, and China. He concludes his intereſting details by enquiring whether this ſubſtance exiſts in other countries, except thoſe acknowledged to be the chief, if not the excluſive ſituations of this ſpecies. This queſtion ariſes from ſeveral ſtones found in Europe having been given as corindons. In fact, thoſe collected in Germany were found to be ſometimes feldſpars, and ſometimes the *ſchorlartiger-beryll* of Werner, your leucolite. That mentioned in the Muſeum Brittanicum as coming from Tyrie, on the eaſtern coaſt of Scotland, was far from having the hardneſs belonging to this ſpecies; that from Cheſnut-hill near Philadelphia, announced by Mr. Smith, was diſcovered by Mr. Richard Phillips to be a fragment of badly cryſtallized quartz. It only remained to decide on the feldſpar found by Bournon in France, in the province of Forez, the deſcription of which he ſent to you in a letter inſerted in the Journal de Phyſique for June 1789, and which he ſtill conſiders as a true corindon.

The ſpecimens of European ſtones called corindon do not belong to that ſpecies.

This ſubſtance appears to be the ſame as that you have called andalouſite, and which ſome dealers in natural hiſtory have circulated in commerce by the name of adamantine ſpar, from the kingdom of Caſtile: it has been placed by Abbé Hauy in the appendix, which contains thoſe ſubſtances, the nature of which did not appear to be ſufficiently known to permit him to aſſign them a place in his method. He calls it apyrous feldſpar. Thus we may be ſatisfied that hitherto there is no certainty of corindon having been found in Europe. I flatter myſelf that now there will be no doubt on the corindon of Italy which I have the honour to announce; it is not even deficient in the ſpecific gravity belonging to this ſubſtance; a defect which induced Profeſſor Hauy not to acknowledge the feldſpar of Forez to be a corindon.

If

Probability that Europe may afford the precious gems.

If rubies and ſapphires be claſſed with corindon, as many mineralogiſts ſeem diſpoſed to do, Europe will probably have mines of theſe precious ſtones as it already has mines of emeralds, ſuch as thoſe of Limoges, diſcovered by the learned Lelievre, counſellor of the mines. But theſe ſtones will be rubies, ſapphires, and emeralds of mineralogiſts, but not of the jewellers, until they ſhall be found very tranſparent, which I hope will be the fruit of new reſearches, followed with perſeverance.

HERMENEGILDE PINI.

IX.

On diſcloſing the Proceſs of Manufactories. In a Letter from Mr. John Clennell.

To Mr. NICHOLSON.

My Dear Sir, *Newcaſtle, Feb.* 17, 1805.

Intereſting queſtion reſpecting the diſcloſure of manufacturing proceſſes.

T me to intreat the attention of ſome of your numerous correſpondents towards a queſtion which muſt certainly be intereſting to every manufacturer, but of which no regular diſcuſſion has yet been offered—Is it proper or improper to lay before the public in reſpectable periodical works, a full and impartial ſtatement of the various proceſſes of our manufactories? I ſhall ſtate ſuch reaſons as have offered themſelves to me why they ſhould be diſplayed, but I am principally anxious to receive further information on a ſubject that appears to me peculiarly intereſting.

The firſt argument I ſhall adduce is that of Mr. Boyle, as quoted by Dr. Johnſon in the 201ſt number of the Rambler. "The excellency of manufactures, and the facility of labour, would be much promoted, if the various expedients and contrivances which lie concealed in private hands, were, by reciprocal communications, made generally known; for there are few operations that are not performed by one or another with ſome peculiar advantages, which, though *ſingly* of little importance, would, by *conjunction and concurrence,* open new inlets to knowledge, and give new powers to diligence."

Argument of *Boyle,*

The

confirmed by experience.

The ſecond is the very conſiderable improvements that have taken place in thoſe few manufactories which have yet been under the influence of chemical enquiry; thus realizing, but on a very extenſive ſcale, the ſuggeſtions of Mr. Boyle: ſo far therefore as we are to be guided on the one hand by experience, and on the other by the influence of ſcientific enquiry on liberal diſplay, will the argument be in our favour.

Accidental diſcoveries improved by diſcloſure.

In the third place I would obſerve, that as many very valuable diſcoveries are owing to accident, thoſe with whom they happen are frequently perhaps incapable of improving them to the extent they would admit of in the hands of men of ſcience, and thus, by a ſpirit of monopoly, preclude even themſelves from the advantageous cultivation of ſuch diſcoveries, merely leſt others might enjoy it alſo. If, again, we conſider the rapid progreſs that has been made of late years in every department of uſeful and practical knowledge, we muſt attribute it entirely to thoſe liberal communications that have been made by men whoſe attention has been immediately directed to the promotion and improvement of every thing valuable to the public. Again, the profits of every buſineſs depend on the regularity and knowledge with which it is conducted; but how is the laſt to be enjoyed without reſources to apply to, and how much more eaſily would it be obtained if ſcience could regulate and ſimplify the combinations of the manufacturer? If to accompliſh by every thing employed (and even in many caſes the refuſe) in each proceſs its utmoſt poſſible uſe, is a favourite principle with manufacturers; to take the moſt accurate and beſt adapted means to effect it, ought certainly to be as powerful with them. Is it not alſo obvious, that, to diſcard all myſtery and quackery, and fairly to diſcloſe each proceſs, is to invite the attention of men of ſcience and reſearch to extend any advantages gained by chance or otherwiſe, and to diſcover greater utility in the various ſubſtances employed. The origin, progreſs, preſent ſtate, and hints for the improvement of the "arts of life," would certainly be worthy the contemplation of our firſt chemiſts, and are ſubjects that have appeared of ſuch importance to a neighbouring nation, that many of their moſt eminent men have been employed in them; and ſome volumes of the *Encyclopedie Methodique* are dedicated to ſuch information, with plates too, in ſeveral inſtances diſplaying even the moſt minute work-tools employed in each.

Science would be thus introduced into workſhops, &c.

Some objections anſwered.

The hiſtory and detail of manufactories conducted in each place ought, I preſume, to form a principal object with the writers of local hiſtories; yet very few of thoſe gentlemen are enabled to obtain ſuch accounts as they can depend on, from the ſelfiſh and monopolizing jealouſy of manufacturers in general. To theſe various advantages an objection may be offered, "That diſplay is placing objects of taxation in the hands of miniſters: be it ſo; diſplay will make it eaſier to collect the tax, will make it more certain, and it may be, *leſs oppreſſive*: if to theſe be added the above advantages, it may fairly be preſumed, that diſcovery and conſequent improvement is the moſt advantageous track to be purſued; but, my dear Sir, I beg your pardon, on this ſubject I did not mean to offer my own opinion ſo much as to ſolicit information from that of others.

I am truly your's,

JOHN CLENNELL.

Literary purſuits are compatible with the duties of merchants and manufacturers.

How far literary purſuits are compatible with the duties of the commercial man, or the manufacturer, ſeems a queſtion ſo completely decided in the affirmative, in the firſt volume of the Mancheſter Memoirs, by Mr. Henry; in the ſecond volume of the ſame work, by Dr. Barnes; and in the hundredth number of the Lounger,—that the above paper aſſumes the principle as being fully eſtabliſhed. J. C.

X.

Queſtion reſpecting the Purification of Copper. By J. P. *With a conciſe Reply.* W. N.

Queſtion reſpecting the purification of copper.

AS copper in its pureſt ſtate (eſpecially out of London) for manufacturing different articles, cannot be obtained without a very tedious proceſs; as it forms the principal ingredient in mirrors for reflecting teleſcopes, and likewiſe is much uſed as an alloy for gold; if it is impure, it never fails to render the gold ſo alloyed brittle, and not to be reſtored to its ductility until the impure alloy be waſted by ſubſequent meltings, to the loſs and diſappointment of the workman. Required, therefore, a method of purifying the copper, particularly for the latter purpoſe?

Your's reſpectfully,

Morley Street,
Newcaſtle-upon-Tyne.

J. P. Jun.

Reply.

As the proceſſes for refining copper in the large way are grounded upon its property of refiſting oxidation more than the other metals which are uſually combined with it, it may be adviſeable to adopt the proceſs of Pelletier, with a due attention to the manipulation and the proportion of mangancſe to be made uſe of. The very intereſting letter of Mr. Thomſon in the preſent Number, will indicate the principles of operation. Mr. Hatchett's excellent papers in the Philoſophical Tranſactions, of which a correct abridgement is given in our V. and VI. Volumes, ſhew the miſchievous conſequences of impurity in the copper for alloying the precious metals; and it is but too well known, that it is difficult to be procured, or even to be made pure, upon a ſcale of extenſive magnitude.

W. N.

XI.

Deſcription of a Method of connecting Iron Bars, and coating them with Lead, ſo as to form ſolid Pillars for Light-houſes on Rocks covered at High-water, and to defend them from Corroſion. By Capt. JOSEPH BRODIE, *of the Royal Navy.*[*]

Deſcription of the means of coating iron bars: by reference to the drawing.

FIG. 1, *Plate V.* A ſhews four rods of caſt iron, compoſed of a number of pieces two feet long, rivetted together, in a manner explained by the plate, ſo as to produce the effect of one bar of the thickneſs of the whole. B. A tube of caſt iron, formed from a number of ſeparate pieces, each about ten inches long, and which, when placed round the iron rods above-mentioned, and then ſcrewed together, form a mould, into which the melted lead is to be poured, to coat the iron rods. C. A portion of the rods covered with the melted lead, ſo as to form a cylindrical pillar apparently of lead, the iron being perfectly coated therewith.

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[*] Communicated to the Society of Arts (Memoirs, MDCCCIV. 258.) who voted him the gold medal.

Fig. 2. D ſhews the manner in which the hollow cylinder is formed to any length required, by the junction of a number of ſemi-cylinders rivetted together and fitting each other. E, the ſide flanges ſcrewed cloſe together. F, the end flanges alſo ſcrewed together, as prepared for the melted lead.

After a certain portion of the iron rods are coated with lead, the lower parts of the tube are taken off and placed higher up; by which repeated changes, a few tubes will anſwer the purpoſe to coat any length of the iron rods.

XII.

Reply to Mr. ACCUM's *laſt Letter on the Production of Nitrous Acid. By* W. F. S.*

To Mr. NICHOLSON.

SIR,

Remarks on Mr. Accum's letter.

I THINK Mr. Accum by no means throws off the charge of cenſure with which he is accuſed, upon ſo material a part of the modern theory. He ought to have been more explicit, and in ſo nice and ſo diſputed an experiment, to have given us a minute detail.

Unſucceſsful attempts to repeat the experiment of forming nitrous acid.

I have performed the experiment which he ſpeaks of, but I could detect no nitrous acid after the proceſs. The air gradually diminiſhes by the electric ſpark, but this diminution is owing to the oxigen gas producing a calcination of the metals employed, for the pureſt oxigen gas anſwers better than when mixed with nitrogen. Therefore I hope, if Mr. Accum poſſeſſes a more accurate experiment, he will give it through your Journal: certainly the preſent ſtate of modern chemiſtry requires a very minute inveſtigation. I hope, Mr. Nicholſon, you will not refuſe inſerting this in your Journal.

London, May 2.

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* See our Vol. X. p. 109 and 211.

Experiments

XIII.

Experiments on the Electricity of Metallic Filings ſifted through Metal; with Remarks in anſwer to a Letter of Mr. Cuthbertſon. By Mr. Wm. Wilson. (*From the Author.*)

To Mr. NICHOLSON.

SIR,

An error pointed out by Mr. Cuthbertſon admitted.

IN the laſt number of your Philoſophical Journal, I find a letter from Mr. Cuthbertſon, containing ſome remarks on my letter on the electricity of metals, in which he notices an error in the table of reſults of experiments, that had eſcaped my notice till I read his letter. In that table I have by miſtake put P againſt copper filings ſifted through zinc inſtead of N.

Copper filings when ſifted through zinc are electrified ſtrongly with negative electricity.

Mr. C.'s objection to the production of electricity by mere ſeparation of metals, conſidered.

The metallic filings *touch* the ſieve, are *ſeparated*, and *touch* the receiving metal.

As the el. varies with the ſieve it does not depend on the *latter contact*,

—and in the *firſt contact* there is no inſulation,

—conſequently it depends on the *ſeparation*.

Mr. C. in his other remarks does not ſeem to admit that the ſeparating the metals from contact is the cauſe of the electrical fluid being excited and not touching, becauſe both touching and ſeparating are employed. In the way the experiments were made touching takes place two ways, viz. the filings come in contact with the metal plate they are ſifted into, and they are in contact with the ſieve before they are ſifted through it; but I cannot conceive how either of theſe contacts could cauſe the excitation of the electricity in the experiments. If the contact of the filings with the plate they were ſifted into was the cauſe, we ſhould have had the ſame effect with the ſame filings, whatever metal the ſieve was made of they were ſifted through; becauſe as they were always ſifted into the ſame plate, the ſame metal filings always came in contact with the ſame metal plate, and conſequently we ſhould have had the ſame effects in all caſes with the ſame filings; whereas out of the ten ſorts of filings that were uſed, there was only one (ſteel) that produced the ſame effect with the different ſieves; and if the contact of the filings with the ſieve excited any electric fluid, it would be diſſipated as faſt as excited, becauſe neither the ſieve nor the filings were inſulated; conſequently it could have no effect on the reſults of the experiments.—Therefore ſince neither of theſe contacts excited the fluid, it muſt have been excited by the ſeparation.

I think the following experiments will put this beyond a doubt: 1. I faſtened a piece of card into a ſtick of glaſs, and then rubbed it over with ſtrong gum water and covered it with filings of zinc; ſo that when it was dry, it had a ſurface of filings of zinc. From a heap of the ſame filings I took up as much as I could on this little ſhovel without touching it with any thing elſe, and let them fall very ſlowly upon a piece of bright ſheet copper faſtened in an inclined poſition upon the cap of an electrometer, and formed into a receptacle at its lower part to contain the filings. In this operation almoſt every particle of filings neceſſarily came in contact with the face of the copper. After letting fall upon the copper about an ounce and a half of filings there was not the leaſt ſenſible effect on the electrometer. 2. I then took a piece of the ſame ſheet copper, which was pierced full of ſmall holes, and ſifted through it the ſame filings I had uſed in the above experiment, upon the ſame copper on the electrometer, and the gold leaves diverged with poſitive electricity and diſcharged themſelves againſt the ſlips of tin foil on the inſide of the glaſs ten times before the whole of the ounce and half of filings were ſifted into it.

An inſulated ſhovel of card, faced with zinc filings was uſed to pour zinc filings on a bright plate of copper.

The copper plate was not electrified.

A copper ſieve was then uſed inſtead of the ſhovel.

The copper plate was ſtrongly poſitive.

Now, ſince the ſeparating the two metals from contact in the ſecond of the above experiments is the only difference between them, and as the electric fluid was excited only in the ſecond, I think we may ſafely conclude that that ſeparation was the cauſe of the excitation, and not touching.

Separation was alone the cauſe of this electricity.

I am very much inclined to believe that the excitation that takes place in friction is cauſed by the ſame circumſtance, and that the friction does nothing more towards the excitation than bring the different parts of the ſubſtance that are rubbed together into contact, and ſeparate them from it.

Electric excitation is probably a fact of this kind.

I am your obedient humble ſervant,

WILLIAM WILSON.

Reply

XIV.

Reply to Mr. Boſwell. By An Old Correspondent.

To Mr. NICHOLSON.

SIR,

Explanatory remarks on Mr. Boſwell's letter.

WHEN I began to read your correſpondent, Mr. Boſwell's anſwer to my obſervations on his geometrical propoſitions, and found myſelf accuſed of having "thrown ſome very undeſerved reflections" on his communication, I could not help feeling a degree of apprehenſion leſt I ſhould inadvertently have made ſome miſtake or other, for which I muſt have been obliged to apologize to him and to the public; but on peruſing the letter through, I was not a little ſurpriſed to have found but *one* reflection pointed out, and that applying not to the *matter* but *manner* of my obſervations; it ſeems I have accuſed him of being *too confident* in one of his aſſertions; a literary crime, of which he exculpates himſelf by proving from a quotation, certainly very much to the purpoſe if we make no diſtinction between *doubt* and *diffidence*, that he is, on the contrary, a very *diffident writer*, which quotation, to be ſure, contraſted with an expreſſion that fell from my pen, might be concluſive, if the two paſſages were applicable to the *ſame thing;* but unfortunately it turns out, on cloſer examination, that the accuſation applies *excluſively* to Mr. B's *ſecond* propoſition, and his exculpatory quotation *excluſively* to his *firſt.*

Had I committed myſelf ſo far as to ſay that he announced his firſt propoſition with confidence, I muſt have ſtood clearly convicted of having done him injuſtice; but as it was his ſecond propoſition, or, in other words, his *other fact* in geometry, to which my objectionable obſervations *ſolely* applied, he ought to have quoted what he has ſaid about *it,* and about *it only,* as evidence againſt me; he has, however, directed his arrow at a wrong mark, on which account I claim the reader's indulgence to repeat the paſſage alluded to, which is the *only* paſſage in Mr. B's paper that relates to his ſecond propoſition: "The diſcovery of a fact in geometry often leads to *another;* one of this kind *I have here to add, which is,* that a right line (B E) drawn from the extremity B of the line I B, at right angles through the oppoſite diameter (I F) to the circumference, Explanatory remarks on Mr. Boſwell's letter.

ence, *will be equal* to a fourth of the circumference." In this annunciation, which is an appendage to the main ſubject or firſt propoſition, and evidently not included in the prefatory apology, I ſee nothing like a ſuſpicion of inaccuracy expreſſed, or even hinted at, and conſequently can trace no mark of *diffidence*, now that I come to examine it again; on the contrary, I repeat, that a fact is confidently aſſerted to exiſt, which has been proved *not to be a fact*, both by myſelf and by Mr. Gough, to whoſe teſtimony probably the reader will pay ſome deference, particularly as this gentleman has ingeniouſly ſhown that *another line* in the circle poſſeſſes the identical property erroneouſly attributed to the line B E, and that previouſly to Mr. B.'s complaint being made public: to the reader, therefore, the falſeneſs or juſtice of my obſervation or "reflection" muſt be referred; and it will anſwer the purpoſe of both Mr. B. and myſelf, if he will let the affair drop here; for *he* will then ſtand a good chance of being reputed what is his principal aim, a *diffident writer*, and I ſhall ceaſe to be, what ſome of your readers, beſides Mr. B. may poſſibly be diſpoſed to think me, a *caviller*.

I am, Sir, once more,

AN OLD CORRESPONDENT.

May 17, 1805.

P. S. The mathematical portion of your readers need not be informed, that the *forgetfulneſs* imputed to me, reſpecting the aſſumed *approximation* ſubſtituted for the *exact ratio* of the diameter to the circumference of a circle, is a charge applying with equal propriety to every mathematician who has deduced calculations depending on the area of a circle: even Mr Gough, whoſe mathematical ſkill is juſtly the admiration of thouſands, whom he has never ſeen, and is doomed never to ſee, has ſomehow been *obliged* to be guilty of the ſame want of recollection, though his calculations may be conſidered as the reſult of *demonſtrative truth*.

It remains now for Mr. Boſwell to ſhow the ſcientific world, by an example or two, how he applies his diſcovery to practical meaſurements, which it is preſumed, will prove a communication of general intereſt.

XV.

Deſcription of an accurate Method of banking the Balance of a Time-keeper. By Mr. WILLIAM HARDY. *Extracted from his Letter to* CHAS. TAYLOR, *Eſq. Sec. to the Society of Arts.**

SIR,

Importance of the ſtop or banking piece uſed to prevent extreme vibrations in time-pieces.

THIS letter is accompanied with a drawing, a deſcription, and a model, of a more perfect mode of banking the balance of a time-keeper, than any that has yet appeared; and its application to a time-keeper is a matter of ſuch real importance, that the moſt accurate, without this moſt neceſſary appendage, is liable to ſuch derangement, that from the moſt trivial cauſe it is in one moment rendered uſeleſs.

The author's invention is confirmed by long trial,

To preſerve the good qualities of the time-keeper, on which often the ſtrength, the wealth, the grandeur, and ſafety of this great empire depend, I deem it neceſſary that my invention ſhould be laid before the Society of Arts, as the means of its being more generally known; and I hope that I ſhew proper reſpect to the Society, when I aſſure you that I do not offer any crude idea, neither could I think of giving you any trouble, until I had fully verified the utility of my contrivance by ſeveral years trial. As I can produce the teſtimony of

and teſtimony.

ſome of the moſt eminent watchmakers in favour of my invention, I look forward with ſome degree of confidence, in expectation of obtaining the approbation of the Society.

The banking is required in watches which have a vibration through very great arcs.

It was at firſt imagined, that a banking to a watch with a free eſcapement was quite unneceſſary, as the limits of banking were ſo great as to admit of almoſt twice 360, or 720 degrees; but, on trial, the balance was frequently found to exceed this quantity, and that a very ſlight motion given to the time-keeper (particularly when the axis of the balance became the axis of that motion), was ſufficient to alter the ſtrength and figure of the pendulum-ſpring, and poſition of the pieces in reſpect of the balance-wheel, ſo as to change the rate of the time-keeper; and, what was worſe, require a new adjuſtment of the balance, to accommodate itſelf to the changes made in the ſpring, and other parts connected with it. Hence it became neceſſary, that ſome means ſhould be uſed to ſtop the balance at certain limits beyond its natural arch of vibration; and various attempts have been made to effect it. One way is, by a moveable piece on the axis of the balance, which banks againſt a pin, yet ſo as to ſuffer the balance to vibrate more than 360 degrees. Another method is to have a piece moveable on a centre in one of the arms of the balance, and applying itſelf as a tangent to the pendulum-ſpring, which paſſes through a hole in the piece. It has alſo a knee, which almoſt touches the plate, and juſt paſſes free of a pin placed in it. But when the balance vibrates ſo as to approach its utmoſt limits, the action of the ſpring, while in a ſtate of unwinding, throws the piece outward, ſo as to fall in the way of the pin, and ſtop the balance from proceeding farther. Another mode is by a ſtraight ſpring, ſcrewed upon the plate, having a hook at the end of it, into which a pin placed in the balance ſtrikes, when, as before, the pendulum-ſpring, in unwinding, touches the ſtraight ſpring. and moves it a little outwards. There is alſo a way of banking by means of a bolt, which is thrown back by the pendulum-ſpring, and made to fall in the way of a pin placed in the rim of the balance. Theſe are the principal modes of banking now in uſe, and they do not differ materially from one another in principle. But the weight and friction of ſo many pieces, on ſo delicate an organ as that of a pendulum-ſpring, are perhaps nearly as hurtful to the time-keeper as the injury it may ſuſtain when it is left without any banking whatever.

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* In their Memoirs for 1804. A premium of 30 guineas was awarded for this invention.

Former methods of banking.

They are objected to from weight and friction.

In *Figures* 1 and 2, *Plate VI.* the ſame letters are placed, to ſignify the ſame things. A A is the balance to which the pendulum-ſpring is faſtened in the uſual way. In one of the croſſes of the balance is placed a pin P, which ſtands a little way above its ſurface; and when the balance is cauſed to vibrate a complete circle, the pin in its motion will deſcribe the dotted circle P O Q, and juſt paſs clear of the inſide of a projection formed on a cock B, which is faſtened on the plate by means of a ſcrew. At about one-fourth of a turn of the pendulum-ſpring, reckoned from its ſtud E, is placed a very delicate tapering piece of ſteel S, having a ſmall hole in

Deſcription, with reference to the engraving.

Deſcription, with reference to the engraving. it, through which the pendulum-ſpring paſſes; and it is faſtened to it by means of a pin, and ſtands perpendicular to the curve of the ſpring. Let the balance be at reſt, as repreſented in *Fig.* 1, the banking-pin at P, and the banking-piece at *s*. Suppoſe the balance is made to vibrate from P towards O, when P arrives at the banking-piece *s*, it will paſs it without touching, becauſe its extremity *s* lies wholly within the circle traced out by the banking-pin. But when the banking-pin P has arrived at Q, the banking-piece *s* will have advanced to *t*, by the pendulum-ſpring winding itſelf up into the figure repreſented by the dotted curve; and when the banking-pin P (now at Q) returns back to P, and paſſes on from P towards Q, to approach B, and ſo complete the other half-arch of its vibration, before P can arrive at the banking-cock B, the pendulum-ſpring will have unwound itſelf into the figure deſcribed by the dotted curve, and the banking-piece *s* will have advanced into the poſition at *r* juſt touching the banking-cock. Its extremity *r*, however, being thrown beyond the dotted circle, muſt neceſſarily fall in the way of the banking-pin, which arrives there almoſt at the ſame moment, and is oppoſed by it, without the ſlighteſt ſhock to the pendulum-ſpring. The model* renders any farther explanation unneceſſary.

WILLIAM HARDY.

No. 61, *Chapel-Street, near White-Conduit-Houſe,*
Jan. 18, 1804.

XVI.

Deſcription of a new Apparatus for making the gaſiform Oxide of Carbon. Communicated by Mr. DEYEUX.†

The progreſs of chemiſtry owing chiefly to improved apparatus. It is generally admitted, that chemiſtry is indebted to the invention of various kinds of apparatus, and the perfection to which they have been brought, for much of the progreſs it has made within theſe laſt thirty years.

Inſtance in that of Woulfe. For inſtance, before Woulfe made known his apparatus for obtaining the aeriform fluids evolved from various ſubſtances, either when expoſed to the action of fire, or when brought into contact with matters capable of combining with them, the operator was obliged to employ bulky veſſels, difficult to manage, and ſo inconvenient, that he did not wiſh to apply them; and when he did, it was impoſſible for him to collect the aëriform fluids with any accuracy; for when they were rarefied to a conſiderable degree, they eſcaped through apertures left purpoſely to preſerve the veſſels from burſting.

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* Which is preſerved in the collection of the Society.

† Annales de Chimie, Vol. LIII. p. 76.

Inconvenience of the old inſtruments.

Theſe inconveniences are now removed by Woulfe's apparatus, ſo that the operations in which gaſes are evolved may eaſily be performed in veſſels of ſmall bulk; the gaſes may be ſubjected to calculation; their quantity as well as quality aſcertained with the utmoſt preciſion; and operations, which were formerly conſidered as very hazardous to the operator, may now be continued for hours together without the leaſt fear of injury.

Contraſted advantages of the modern arrangement of veſſels.

With theſe advantages many others are connected; and it is known to every one, that they are owing to the degree of perfection to which chemiſts have brought Woulfe's apparatus, and particularly to their happy application of it on various occaſions.

Yet, notwithſtanding theſe diſcoveries have been carried a great way, it is more than probable that many remain to be made: too much praiſe therefore cannot be beſtowed on thoſe who turn their attention to this important object, ſince the apparatuſes they invent are ſo many new means afforded chemiſts of collecting an infinite number of products which frequently eſcape them, and the knowledge of which may have great influence on the improvement of chemical ſcience.

Improvements yet remain to be made.

From theſe motives I have thought it may be of uſe to make known an apparatus juſt invented by Mr. Baruel, operator to the chemical lectures of the Medical School at Paris.

Invention of Mr. Baruel;

This young chemiſt, who had often noticed the difficulties and even dangers incurred when it is neceſſary to decompoſe gaſes, or to combine them with different ſubſtances, attempted to make ſome alteration in the proceſſes commonly employed in the laboratory for operations of this ſort; and after ſeveral trials he invented an apparatus, which ſucceeded beyond his hopes.

for decompoſing gaſes, or combining them with other ſubſtances.

I have ſeen this apparatus employed with the greateſt ſucceſs for the fabrication of the gaſiform oxide of carbon. This

Gaſiform oxide of carbon formerly procured with difficulty,

gas,

is now eaſily made.

gas, which formerly was to be procured only with difficulty and in ſmall quantity, may now be obtained eaſily, readily, and without much expenſe, which affords the advantage of ſubjecting it to many more experiments than has hitherto been done.

Other uſes of the appa

The apparatus in queſtion may be of uſe likewiſe for the preparation of ſulphurated hydrogen gas, carbonated hydrogen gas, and phoſphorated hydrogen gas: it may likewiſe be employed for ſaturating a gas with any ſubſtance whatever, particularly when a high temperature is requiſite for this ſaturation.

The better to make known the apparatus of Mr. Baruel, I will give the deſcription of it communicated to me by the author, and add a ſketch of it, which will give a more complete idea of all its parts. *Plate VIII.*

Deſcription of the apparatus, and method of procuring gaſiform oxide of carbon.

Suppoſe it is required to make gaſiform oxide of carbon: ſome charcoal is to be taken very dry and carefully choſen, broken into ſmall pieces, and introduced into the three gun-barrels, B, C, D. The charcoal is to be preſſed lightly together with an iron rod, ſo as to occupy only the part of each barrel that is to be heated, but it muſt not be rammed hard. The three gun-barrels are then to be placed horizontally ſide by ſide in a reverberatory furnace, A, leaving about two inches diſtance between them; ſecured in their places with moiſtened brick earth, and covered with the dome of the furnace.

This done, fix in the mouth of the barrel B the glaſs tube E, which is curved ſo as to admit its other extremity to be inſerted into the neck of the bottle F; the neck of this bottle being large enough to hold likewiſe the pipe of the curved funnel G. Into the oppoſite end of the barrel B one of the ends of the curved tube H is to be introduced, the other end of the tube being inſerted into the opening of the barrel D, ſo as to form a communication between B and D. A ſimilar tube I is fitted to the other extremity of D and the adjacent end of C, forming a communication between theſe two barrels. Laſtly, From the oppoſite extremity of the barrel C iſſues the tube K, which is bent at right angles, ſo that its ſecond curvature paſſes under the receiver M, placed on the ſhelf of the pneumatic tub with water L.

Every thing being thus arranged, carbonate of lime diluted with a ſmall quantity of water is to be poured into the bottle F, and after all the joints of the tubes have been luted with great care, a fire is to be kindled in the furnace. As ſoon as this fire is ſufficiently ſtrong to make the gun-barrels red-hot, ſulphuric acid is to be poured into the funnel G, and, when it comes into contact with the carbonate of lime in the bottle F, it will expel a large quantity of carbonic acid. This acid preſently paſſes through the tube E into the barrel B, is conveyed from B to D through the tube H, from D to C through the tube I, and thence iſſuing by the tube K, comes out beneath the receiver M, placed on the ſhelf of the pneumatic tub.

Deſcription of the apparatus, and method of procuring gaſiform oxide of carbon.

The object propoſed by Mr. Baruel in this arrangement of his apparatus, was to oblige the carbonic acid gas evolved from the carbonate of lime, to traverſe the charcoal contained in the three gun-barrels, and thus ſaturate itſelf with all the carbon it could take up.

In fact it is eaſy to conceive, that this method muſt be more certain and expeditious than that formerly employed, when the operator was ſatisfied with paſſing the gas through a ſingle barrel. It is true it was collected, and ſubjected to a ſecond operation, or even to a third; but this mode was tedious, and much of the gas was always loſt. In this new method on the contrary nothing is loſt, and a product is ſeparate[illegible]nce, which poſſeſſes all the properties that characteriſe the gaſiform oxide of carbon, and which may be uſed unſparingly, ſince it is always obtainable in large quantity.

This method more certain and expeditious than the old; for nothing is loſt, and the product is obtained at once.

XVII.

Deſcription of an improved Mill for levigating Painters Colours.

By Mr. JAMES RAWLINSON, *of Derby.**

THE hitherto very unmechanical, inconvenient, and highly injurious method of grinding poiſonous and noxious colours, led me firſt to imagine a better might eaſily be contrived for that pur[illegible]t muſt be obvious to every perſon, that the

Great inconveniences and unwholeſome effects of grinding colours on the ſtone.

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* From the Memoirs of the Society of Arts for 1801, who awarded him the ſilver medal.

method hitherto adopted of grinding colours on an horizontal marble ſlab, with a ſmall pebble muller, requires the body of the perſon who grinds to bend over that ſlab, and conſequently his head; which cauſes him conſtantly to inhale the noxious and poiſonous volatile parts of the paint, which is not unfrequently ground with oil ſaturated with litharge of lead; and if we may judge from the very unhealthy appearance of theſe men, accuſtomed to much colour-grinding, it ſhould ſeem the bad effects of this employment require a ſpeedy remedy.

Machine by which the work is much better performed, and without inconvenience.

It works with five times the ſurface of the muller,

The machine, of which I now ſend the Society a model, has not only the advantage of being an effectual remedy of this extenſive and ſevere evil to recommend it, but it grinds the colour much eaſier, much finer, and much quicker, than any method hitherto adopted. Having occaſion for a conſiderable quantity of colour-grinding in the profeſſion in which I am engaged, and that in the fineſt ſtate poſſible, and having made uſe of this machine for ſeveral years, and being more and more convinced of its utility, I thought it my duty to preſent it to the Society of Arts, hoping that it might not be altogether unworthy of their attention. The roller of the machine that I uſe is ſixteen inches and a half in diameter, and four inches and a half in breadth. The concave muller that it works againſt covers one-third of that roller: it is therefore evident, that with this machine I have ſeventy-two ſquare inches of the concave marble muller in conſtant work on the paint, and that I can bring the paint much oftener under this muller in a given ſpace of time, than I could by the uſual method with the pebble muller, which is ſeldom more than four inches in diameter, and conſequently has ſcarcely ſixteen ſquare inches at work on the paint, when my concave muller has ſeventy-two. I do not mean to ſay that a roller, the ſize of that which I now uſe, is the largeſt which might be employed; for truly I believe that a roller two feet in diameter, with a concave muller in proportion, would not be hard work for a man; and then the advantage to the public would be ſtill farther increaſed.

and is applicable to water-colours as well as thoſe in oil.

This machine will be found equally uſeful for the colours ground in water, as for thoſe ground in oils; and I doubt not but the great importance of this ſimple machine will be very ſoon generally experienced in all manufactories where colours are uſed. The labour neceſſary with this machine, in grinding colours exceedingly fine, is very eaſy. It is uſeleſs to enter into any minute deſcription in this place, as a bare inſpection of the machine muſt ſufficiently explain itſelf.

To the colourman it would evidently be an eſſential ſaving of labour, and conſequently of expenſe, which will probably have ſome weight as a recommendation; and the advantages to the colour-grinder have been already ſtated.

Deſcription, by reference to the drawing.

Plate V. Fig. 3. A is a roller or cylinder made of any kind of marble; black marble is eſteemed the beſt, becauſe it is the hardeſt, and takes the beſt poliſh. B is a concave muller covering one-third of the roller, of the ſame kind of marble, and fixed in a wooden frame *b*, which is hung to the frame E at *i i*. C is a ſtrong piece of iron, about an inch broad, to keep the muller ſteady, and is fixed to the frame with a joint at *f*. The ſmall binding-ſcrew, with the fly nut, that paſſes through the centre of the iron-plate at *c*, is for the purpoſe of laying more preſſure on the muller, if required, as well as to keep it ſteady. D is a taker-off, made of a clock-ſpring about half an inch broad, and fixed in the manner of a frame-ſaw in an iron frame *k*, in an inclined poſition to the roller, and turning on pivots at *d d*. G is a ſlide-board to draw out occaſionally, to clean, &c. if any particles of paint ſhould fall from the roller, and which alſo forms itſelf for the plate H, to catch the colour on as it falls from the taker-off. F is a drawer, for the purpoſe of containing curriers ſhavings, which are the beſt things for cleaning paint-mills.—E is the frame.

Pulverization, or dry grinding, previous to levigation.

Previous to the colour being applied to the mill, I ſhould recommend it to be finely pulverized in a mortar, covered in the manner of the chemiſts when they levigate poiſonous drugs *. This proceſs of dry-grinding is equally neceſſary for the marble ſlab now in uſe; after which it ſhould be mixed with oil or

water,

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* Or rather in an improved mill, uſed at Mancheſter by Mr. Charles Taylor, for grinding indigo in a dry ſtate, of which I have annexed a drawing, and reference, to render the whole buſineſs of colour-grinding complete.—*Note of the Author.*

This is the ſame apparatus as was uſed under the name of *a philoſophical mill*, in the laboratory at Gottorp, about the beginning of the laſt century. See the memoir of Dr. Joel Langelot, with an engraving, in Lowthorp's Abridgment of the Philoſophical Tranſactions, III. 318.—W. N.

Method of working.

water, and with a ſpatula or pallet-knife put on the roller, near to the top of the concave muller, and the roller turned round, which takes the colour under the muller without any difficulty, and very few turns of the roller ſpread it equally over its ſurface. When it is perceived ſufficiently fine for the purpoſe required, it is very eaſily taken off by means of the taker-off deſcribed, which muſt be held againſt the roller, and the roller turned the reverſe way, which cleans it very quick and very completely; and the muller will only require to be cleaned when you deſiſt or change the colour. It is then turned back, being hung on pivots to the frame at *i i*, and cleaned with a pallet-knife or ſpatula very conveniently. Afterwards, a handful of curriers ſhavings held on the roller, with two or three revolutions cleans it effectually; and there is leſs waſte with this machine than with any marble ſlab.

Quantity and fineneſs of grinding.

As to the quantity ground at once on this mill, it muſt be regulated by the ſtate of fineneſs to which it is required to be ground. If it is wanted to be very fine, a ſmaller quantity muſt be put on the roller at a time; and as to time requiſite for grinding a given quantity of colour, this will alſo depend on the ſtate of fineneſs to which it is ground. I have obſerved that my colour-grinder has ground the quantity of colour which uſed to ſerve him per day, with this machine, in three hours, and, as he ſaid, with eaſe. The colour alſo was much more to my ſatisfaction than in the former way, and attended with leſs waſte.

I have mentioned the pulverizing the colours in a covered mortar, which would prevent waſte, and prevent the duſt and fineſt parts of noxious colours from being injurious to the grinder. In ſome manufactories, where large quantities of colours, prepared from lead, copper, and arſenic, are uſed, this precaution is particularly neceſſary. I do not mean to ſay that my machine is intended to ſuperſede the paint-mill now in uſe for coarſe common colours. It is intended for no ſuch purpoſe; but to ſuperſede the uſe of the very awkward and unmechanical marble ſlab now in uſe, and on which all the colours for china manufactories, coach-painters, japanners, and colour-manufacturers for artiſts, &c. &c. are now ground.

This mill is not a crude project, but has been uſed ſeveral years.

Several of the colour-manufacturers have expreſſed to me their great want of ſuch a machine; and that I had no deſire of troubling the public with a machine that would not anſwer, is evident, from my having uſed it ſeveral years before I preſumed to recommend it to their attention. Being therefore now completely convinced of its utility, and hoping that it might relieve a number of my fellow-creatures from a dangerous employment, I have ventured to commit it to the protection of the Society of Arts; hoping, through their means, to ſee its ultimate ſucceſs. And, farther to give the Society the moſt complete aſſurance in my power, I have annexed the opinion of a very ingenious and mechanical friend of mine who has frequently ſeen it work. If any other queſtions ſhould occur to the Committee, that may be in my power to explain, I ſhall gladly do ſo.

I am, Sir,
Your moſt obedient ſervant
JAMES RAWLINSON.

Derby, Feb. 6, 1804.
Charles Taylor, Eſq.

Inſtructions for tying up the bladders of colour.

P. S. When the colour is ground, I recommend the following mode of tying it up in bladders, in preference to the uſual method. Inſtead of drawing the neck of the bladder cloſe, in the act of tying it, inſert a ſlender cylindrical ſtick, and bind the bladder cloſe around it. This, when dry, will form a tube or pipe, through which, when the ſtick is withdrawn, the colour may be ſqueezed as wanted, and the neck again cloſed by replacing the ſtick. This is not only a neater and much more cleanly mode than the uſual one of perforating the bladder; and ſtopping the hole with a nail, or more commonly leaving it open, to the prejudice of the colour; but the bladder, being uninjured, may be uſed repeatedly for freſh quantities of colour.

N. B. The barrel of a quill may be tied, in place of the ſtick, into the neck of the bladder, with its cloſed end outwards, which will keep the colour ſecure in travelling, and when uſed, the end of the quill being cut off, it may afterwards be cloſed by a ſtick.*

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* A certificate from Mr. Thomas Swanwick, of Derby, and alſo from Mr. John Middleton, of St. Martin's Lane, confirming the above ſtatement, accompanied theſe papers.

Reference to the improved Mill for grinding Indigo, or other dry Colours.

Deſcription of the mill for dry grinding.

Plate V. Fig. 4. L repreſents a mortar made of marble or hard ſtone; one made in the common way will anſwer. M, A muller or grinder, nearly in the form of a pear, in the upper part of which an iron axis is firmly fixed, which axis, at the parts N N, turns in grooves or ſlits, cut in two pieces of oak projecting horizontally from a wall, and when the axis is at work, are ſecured in the grooves by iron pins, O O. P, the handle, which forms a part of the axis, and by which the grinder is worked. Q, the wall in which the oak pieces N N are fixed. R, a weight which may occaſionally be added, if more power is wanted.

Fig. 5. ſhews the muller or grinder, with its axis ſeparate from the other machinery; its bottom ſhould be made to fit the mortar. S is a groove cut through the ſtone.

On grinding indigo, or ſuch ſubſtance, in a dry ſtate, in this mill, the muller being placed in the mortar, and ſecured in the oak pieces by the pins, the indigo to be ground is thrown above the muller into the mortar; on turning the handle of the axis, the indigo in lumps falls into the groove cut through the muller, and is from thence drawn under the action of the muller, and propelled to its outer edge within the mortar, from whence the coarſer particles again fall into the groove of the muller, and are again ground under it; which operation is continued, till the whole of it is ground to an impalpable powder; the muller is then eaſily removed, and the colour taken out.

A wood cover, in two halves, with a hole for the axis, is uſually placed upon the mortar, during the operation, to prevent any loſs to the colour, or bad effect to the operator.

XVIII.

A new and cheap Method of purifying Gold and Silver. By ANDREW THOMSON, Eſq. of Banchory, near Aberdeen. In a Letter from the Diſcoverer.

To Mr. NICHOLSON.

SIR,

I INTENDED to have deferred the preſent communication till ſuch time as I ſhould have it in my power to lay before the public the complete ſeries of experiments in which I have been engaged with regard to the purification of gold and ſilver. But unluckily I mentioned a few particular circumſtances with regard to them, to a man who took it upon him, without my knowledge, to ſend an account of them for publication to a periodical work. As I underſtand that work will not appear ſo ſoon as your next number, I beg, if you think it worthy of a place, that you will inſert the following account of ſome attempts I have been making to purify the precious metals.

This publication is prematurely made, becauſe the inventor might elſe be anticipated.

Being much at a loſs for want of a crucible of pure ſilver for the analyſis of ſome minerals, and as all the uſual methods practiced for purifying that metal are very troubleſome, I ſet myſelf to conſider the various operations on metals, in hopes of falling on a more ſimple way of accompliſhing my purpoſe. At length, I found a proceſs of Pelletier's, which promiſed to ſucceed, and mine is merely extending his idea a little further than he did himſelf.

Proceſs of Pelletier for extracting copper from bell-metal.

He was, I believe, employed by the French government to diſcover an eaſy way of ſeparating the tin from copper on bell-metal,* and the proceſs he gave, is this. Upon the melted bell-metal project black oxide of manganeſe in powder, frequently ſtirring the metal till all the tin becomes oxidated by the manganeſe. He adds a caution, not to add too much manganeſe, otherwiſe part of the copper alſo will be deſtroyed.

He oxides the tin by manganeſe; in ſuch proportion as not to affect the copper.

It immediately ſtruck me, that in this way I might be able to oxidate the copper which alloys our ſilver, and upon making the trial I ſucceeded completely; I had ſome impure ſilver rolled out to about the thickneſs of a ſhilling, this I

The author's improved proceſs. Coarſe ſilver was rolled out; then coiled up; bedded in manganeſe; and

coiled

ſtrongly ignited for a quarter of an hour.
All the metal was oxided.

coiled up ſpirally, and put into a crucible, the bottom of which was covered with black oxide of manganeſe. I then added more oxide till the ſilver was covered, and all the ſpace between the coils completely filled. A cover was then luted to the crucible, and a ſmall hole left for the eſcape of oxigen gaſs. When this had been expoſed for a quarter of an hour to a heat ſufficient to melt ſilver, I found the ſurface of the manganeſe brown from the loſs of oxigen; but, where the ſilver had been, the whole was one uniform black powder, without the leaſt appearance of metallic luſtre, ſo that I had no doubt, that even the ſilver was become an oxide.

The whole contents were then put into a larger crucible with thrice its bulk of green glaſs.

I then put the whole contents of the firſt crucible into a ſecond of a larger ſize, into the bottom of which I put a quantity of pounded green glaſs, about three times the bulk of the contents of the firſt crucible, and luted on a cover as before, to prevent the acceſs of any inflammable ſubſtance.

Strong heat fuſed the glaſs, and reduced the ſilver pure, and alone in a button.
This proceſs anſwers equally with gold.

The crucible was then expoſed to a heat ſufficiently ſtrong to melt the glaſs very fluid. Upon cooling and breaking the crucible, I found the ſilver at the bottom perfectly pure, as its oxide alone could part from its oxigen without the acceſs of ſome inflammable ſubſtance. I find this proceſs anſwers equally well for purifying gold, and to me it ſeems to poſſeſs ſome advantages over all the former methods. The materials uſed are cheap, and a large quantity can be refined as ſoon, and as eaſily as a ſmall quantity, by merely altering the capacity of the crucible you uſe.

The metal muſt be in thin or ſmall maſſes.

I tried the ſame operation on gold and ſilver in round maſſes, but found it went on very ſlowly, and what I ſcarcely expected, in the firſt part of the proceſs of oxidating the metals, the remaining metal continued uniformly impure or nearly ſo, until the whole was oxidated.

The proportions, &c. are not here given, becauſe the author has haſtened to communicate his proceſs.

I regret that I have been forced to make this matter public, before I could do it in a manner ſatisfactory to myſelf. I wiſhed to have given the exact proportions of alloy, manganeſe, and glaſs to be generally uſed, and to have aſcertained if there is any truth in the old opinion, that ſaltpetre melted with gold deſtroys a part of it. I ſuppoſe that idea may have arizen from the oxigen given out by the nitre in a high heat, oxigenating the copper contained in the impure gold, which has been the ſubject of the experiment.

Since the above was written, I have been informed that this matter has actually been publiſhed, but know not in what work. I hope you will ſtill have the goodneſs to inſert this as an *original communication*, as I do not think the perſon who has publiſhed it will have the impudence to call it his own, and as Mr. Kirwan, and other celebrated chemiſts long ago adviſed me to publiſh it, I have already ſtated my reaſons for not following ſuch good advice.

He vindicates his claim.

As I have now been forced to appear before the public, I have hopes I ſhall be able to prevail on ſome of my friends to commit themſelves in the ſame way, in the confidence that their labours will be found uſeful to the public.

I am, Sir,

Your's truly,

ANDREW THOMSON.

Banchory, by Aberdeen,
May 5th, 1805.

XIX.

Memoir on the Propagation of Sound. By M. HASSENFRATZ.*

THE production of ſound is aſcribed by all natural philoſophers to the vibration of the molecules of bodies.

Sound produced by vibrations,

The vibration of theſe molecules admits of two kinds of modification; 1ſt, in velocity; 2ly, in magnitude. The firſt of theſe determines the nature of tones; the ſecond, their force or intenſity.

which differ in velocity and in magnitude.

Sound is tranſmitted to the ear by the molecules that fill the medium or interval between the ſonorous body and the organ of the hearing. The movement of the ſonorous body impreſſes on the molecules of the medium an impulſe, which they tranſmit from one to another, till it reaches the ear, with a greater or leſs velocity. In this tranſmiſſion the vibration may undergo two kinds of alteration: 1ſt, in its velocity; 2dly, in its intenſity. In this memoir I ſhall only tranſcribe ſome experiments relative to the velocity of ſound.

The ſonorous body impreſſes its vibrations on ſome medium, the vibrations of which are continued to the ear.

Philoſophers have long been engaged in determining the velocity of ſound, but conſidering the air as the chief medium

The velocity of ſound hitherto examined only by which it is tranſmitted to the ear, they have attended only to its velocity in air, and have employed two different methods to determine this, theory and experiment.

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* Annales de Chimie, Vol. LIII. p. 64.

This velocity uniform in a given medium,

and the denſity of the medium is one of its elements.

Not affected by the height of the barometer.

Blanconi aſſerts it to be leſs in winter than in ſummer.

Derham denies this, but is probably wrong.

Not affected by rain or fine weather.

Theſe two methods have led to the following remarkable reſults: 1ſt, that the velocity of ſound in a given medium, is uniform, whatever its diſtance from the phonic centre, and whatever its intenſity: 2dly, that the denſity of the medium at equal preſſures is one of the elements of the velocity; for it has been found by theory, that the velocity of ſound is the ſame as that of a body falling from half the height of an atmoſphere ſuppoſed to be of equal denſity with the air in the place where the ſound is tranſmitted; and by experiment, that, all other circumſtances being equal, the velocity of ſound is the ſame at different preſſures of the barometer; ſo that it is equal on the ſummit of a mountain and on the ſea-ſhore. In fact, the denſity of the air being proportional to the compreſſing weight, the height of the column of mercury in the barometer, divided by the denſity occaſioned by this preſſure, is a conſtant quantity; and the height of the atmoſphere of a uniform denſity being equal to the total weight of the air divided by its denſity, it follows that the height of the barometer ought to make no difference in the velocity of ſound. Blanconi aſſerts (Comment. Bonon, vol. II. p. 365), that the velocity of ſound is leſs in winter than in ſummer, ſince, according to his experiments, it takes four ſeconds more in winter to traverſe a ſpace of ſixteen Italian miles. Derham affirms, that the velocity of ſound is the ſame whether the air be extremely hot or extremely cold, though his tables of experiments will be found on examination favourable to the opinion of Blanconi; for the greateſt velocity of ſound in them was on the 5th of April, at one o'clock in the afternoon, being three miles in 111 half-ſeconds, and the leaſt velocity on the 12th of February, at ſix o'clock in the evening, being three miles in 122 half-ſeconds. As the experiments on the velocity of ſound undertaken by the Academy of Sciences in 1737 were made at temperatures that exhibit only two or three degrees difference, perhaps it would be well, as Mr. Laplace thinks, if they were repeated at a time of the year when the temperature is very different; for experience has taught us, that this velocity is equal in rainy and in fine weather, ſo that nothing but change of temperature can produce any variation in this reſpect.

Whatever theſe reſults may prove, as the experiments on the velocity and propagation of ſound have been hitherto made in the air alone, it was an intereſting enquiry to determine the velocity of ſound, when tranſmitted by other bodies, and particularly of different denſities with reſpect to air. Mr. Laplace, to whom branches of phyſical ſcience are indebted for improvement, invited me about eight months ago to make experiments on this ſubject, and particularly on the propagation of ſound through ſolid bodies; and the experiments of which I ſhall give an account in this paper were principally made in conſequence of that gentleman's ſuggeſtion.

Experiments on the tranſmiſſion of ſound through the ſtone in quarries.

Making experiments in the quarries beneath Paris on the tranſmiſſion of ſound through long galleries, I cauſed a perſon to ſtrike with a hammer againſt a maſs of ſtone, retiring at the ſame time by degrees from the place of ſtriking, in order to diſtinguiſh if poſſible the ſound tranſmitted by the ſtone from that tranſmitted through the air. Placing my ear againſt the maſs of calcareous ſtone through which theſe galleries are carried, at a ſhort diſtance I diſtinguiſhed two ſounds perfectly ſeparate, one tranſmitted by the air, the other by the ſtone. Both ſounds grew weaker in proportion as I retired from the ſtriking point; but that tranſmitted by the ſtone was weakened much more rapidly than that tranſmitted through the air. In a gallery excavated beneath Rue de la Harpe the ſound tranſmitted by the ſtone ceaſed to be audible at 134 paces diſtance; and in a gallery beneath Rue de St. Jaques at 140 paces.—— Through the air the ſound was tranſmitted to 400 or 440 paces diſtance. The ſound tranſmitted by the ſtone always reached the ear much ſooner than that tranſmitted by the air.

It was conveyed through the ſtone only 140 paces;

—but through the air 440.

It paſſes quickeſt through the ſtone.

Experiment repeated, and the ſound conveyed 150 paces.

Mr. Berthollet, to whom Mr. Laplace imparted theſe reſults, deſirous of being aſſured whether the ſound of a hammer could be tranſmitted through a maſs of ſtone 140 paces thick, requeſted Mr. Gay, by my deſire, to be preſent at my experiments. With this young chemiſt I repeated the experiment of the tranſmiſſion of ſound through ſtone on ſeveral ſeparate maſſes, and he convinced himſelf, that ſound was capable of being tranſmitted through a maſs 150 paces in length.

The propagation of ſound through ſtone long ago obſerved by mechanics.

It was long ago obſerved in working mines, that the noiſe was propagated to a very great diſtance through maſſes of rock; and the line of the ſound heard through the ſtone ſerves on many occaſions to determine the direction in which the gal-

Its velocity apparently equal to that of light.

leries are carried on; but no one had attempted to obſerve, whether the velocity of the ſound tranſmitted by the ſtone differed from that of the ſound tranſmitted by the air. My experiments in the quarries underneath Paris have taught me, that the difference is conſiderable; and when the gallery is ſufficiently ſtraight, to be able to diſcern the motion of the hammer with the eye, no calculable difference can be perceived between the conveyance of the motion to the eye and that of the ſound to the ear.

The differences of the diſtances to which ſound is tranſmitted through ſolid maſſes remarkable.

The diſtance which the ſound of the hammer can be conveyed to the ear varies conſiderably with the nature of the ſtone and the ſeparations or fiſſures in the maſs. Having cauſed a man to ſtrike with reiterated blows againſt an iſolated wall, built of common ſtone of the ſame kind as that in the quarries, and cemented with mortar, the ſound was tranſmitted only thirty paces. Striking in the ſame manner on a parapet of hewn ſtone raiſed on the borders of the Seine, the ſound was tranſmitted 46 paces. Theſe experiments were made in the open air by day, conſequently under circumſtances leſs favourable to the propagation of ſound than when on the calcareous maſſes in the quarries; but the difference between 30 and 46 paces, under the ſame circumſtances, on maſſes differing only in the dimenſions of the ſtones of which they were formed, is very remarkable.

Experiments repeated,

Encouraged by the ſucceſs of my experiments in the quarries, and by the invitation of Mr. Laplace, I attempted to repeat the ſame experiments on different ſubſtances.

on timber.

By the ſide of the high road that leads from the place *de la Concorde* to *Chaillot* along the bank of the Seine, on the ſtone wharf of St. Leir, oppoſite the ſteam-engine of Gros-Caillou, is placed a railing 210 paces in length, formed of 31 pieces of timber, ſeparated by four large poſts. The blow of a hammer at one extremity of this railing was heard diſtinctly at the other, though through the air it was audible only 120 paces. At the diſtance at which both the ſounds were audible, that through the wood was heard long before the other; and when, ſtanding at the greateſt diſtance from the place of the blow, I heard only the ſound tranſmitted through the timber, the velocity of its tranſmiſſion was ſo great, that it was difficult to diſtinguiſh any interval between the perception of the ſound by the ear, and of the motion of the hammer by the eye.

Conveyed farther than in the open air.

Having convinced myſelf, that the propagation of ſound through ſtone and through wood was effected with much greater velocity than through air, and that the time of its tranſmiſſion to ſuch ſhort diſtances as thoſe on which I was able to make experiments was too little for calculation, I was deſirous of knowing whether the velocity of its tranſmiſſion through metallic ſubſtances were the ſame.

Similar experiments and reſults with long bars of iron.

Several experiments on bars of iron fixed on ſolid maſſes, as the bars that hold together the ſtones of parapets, having given me uncertain reſults, I ſought for iſolated bars of ſufficient length to afford ſome certainty. My firſt experiment was made on the upper bars of an iron railing, 34 paces long, erected on one of the walls of the garden of the Legiſlative Body, adjoining to the Place des Invalides. On ſtriking one extremity of this aſſemblage of bars, two diſtinct ſounds were heard at the other end; that tranſmitted by the bars, and that by the air; the former being always heard firſt. The ſame experiment afterwards repeated on bars of different lengths, gave me the ſame reſult; and this reſult is ſuch, that it is impoſſible to diſtinguiſh, at the ſmall diſtances at which theſe experiments were made, any difference between the tranſmiſſion of the motion by light, and that of the ſound by the ſolid medium.

Mr. Gay ſuppoſed he heard two ſounds through the air, firſt a grave, then an acute.

Repeating my experiments on the velocity and propagation of ſound through the maſſes of ſtone in the quarries beneath Paris, in company with Mr. Gay, this young chemiſt imagined that he diſtinguiſhed two ſounds tranſmitted through the air, one grave and the other acute, which reached his ear in ſucceſſion, the graver ſound appearing to have the greater velocity.

Mairan and others had conjectured the ſame.

This reſult, though contrary to the theory of the propagation of ſound, according to which grave and acute ſounds ought to have the ſame velocity, had already been conjectured by ſeveral philoſophers, particularly by Mairan, and was therefore worth confirmation.* For this purpoſe I ſtretched two ſtrings, one of braſs, the other of catgut, ſo as to make them emit two different tones, the firſt one more acute, the ſecond one more grave; then ſtriking both theſe ſtrings at once with the wood of a black lead pencil, the two ſounds, which were confounded together at firſt, appeared to ſeparate at the diſtance of 400 paces in a large gallery of the quarries, and we both imagined we could diſtinguiſh the graver ſound firſt.

Experiment to determine this.

Probable cauſe of fallacy in this experiment.

While obſerving the propagation of ſound in the galleries, I had ſeveral times occaſion to remark, that a ſound at a great diſtance was frequently repeated, either by reflection or by the vibration of the walls, ſo as to cauſe two differerent ſounds to be heard and diſtinguiſhed, which reached the ear in ſucceſſion. As it was poſſible that the difference which appeared to Mr. Gay and me might have been produced by the cauſe here mentioned, I determined to repeat the experiment in the open air.

The experiment repeated in the open air.

The double ſound produced even here by an echo.

Grave and acute ſounds have the ſame velocity.

For this purpoſe I took two glaſs bells, the tones of which were as an octave and a fifth, that is, the ratio of their vibration was as 1 to 3. A hammer was ſo adjuſted as to ſtrike both the bells at once, and make them ſound at the ſame inſtant. Carrying this inſtrument into the fields, I endeavoured to aſcertain whether the two ſounds reached the ear in unequal times. Several experiments repeated in various places, made me believe a long time, that their velocity was unequal; but having obſerved, that on ſome occaſions the ſounds reached the ear at the ſame time, I was led to remark, that whenever I imagined I diſtinguiſhed a difference of velocity, this difference had been occaſioned by a repetition of the ſound, and that frequently very trifling obſtacles, as trees or hedges, were ſufficient to produce this repetition. I repeated my experiments therefore anew in the midſt of plains of greater or leſs extent, as thoſe of Montronge, Grenelle, St. Denis, &c. and whenever I was remote from any obſtacle capable of producing a repetition, both ſounds were heard at the ſame time. With my inſtrument I could diſtinguiſh the ſound of the two bells at the diſtance of 700 paces or 631 yards; whence it follows, that both experiment and theory concur to demonſtrate, that grave and acute ſounds have the ſame velocity.

Concluſions from the experiments.

From the experiments related in this paper it follows: 1ſt, that the velocity of ſound differs according to the medium by which it is propagated: 2dly, that this velocity is much more conſiderable, when it is propagated by ſolid and very denſe bodies, than when by aeriform bodies, and of little denſity: 3dly, that both grave and acute ſounds have the ſame velocity; a reſult to which theory led us.

XX.

A very advantageous Mode of preparing Muriate of Barytes by the Mutual Decompoſition of ſulphurated Barytes and Muriate of Lime. By Mr. GOETTLING.*

MR. DRIESSEN of Groningen firſt obſerved, that muriate of lime and ſulphate of barytes decompoſe each other at a high degree of heat. Trommſdorff verified the experiment of the learned Dutch profeſſor, and applied the principle to the preparation of muriate of barytes. He perceived, however, that the whole of the ſulphate of barytes was not decompoſed in this way, and he thought, that equal parts of the two ſalts were the ſuitable proportion for decompoſing them as completely as poſſible. Mr. Goettling has deemed the ſubject worth farther inquiry, the reſult of which was the proceſs I ſhall now deſcribe.

Drieſſen firſt decompoſed ſulphate of barytes by muriate of lime; and next Trommſdorff.

One part of native ſulphate of barytes in fine powder is to be mixed with half a part of muriate of lime.† This mixture is to be introduced into a Heſſian crucible, which is to be cloſely covered, and brought gradually to a red heat. The matter muſt be kept in a ſtate of incandeſcence a full half-hour, and frequently ſtirred. It is then to be poured out into an iron cone, and after being coarſely powdered, is to be thrown into three parts of boiling water. The veſſel being immediately taken from the fire, the mixture is to be ſtirred occaſionally with a glaſs ſpatula, and the undiſſolved matter is then to be left to ſubſide. The clear liquor being decanted off, the reſiduum is to be poured into a filter of linen of a cloſe texture, and the fluid lightly preſſed out. The reſiduum being again lixiviated with one part of water, is to be ſtrained as before. The liquors are then to be mixed together, and evaporated to a pellicle, to obtain the ſalt by cryſtallization. In this way we ſhall have five-eighths of a part of muriate of barytes. The

Mr. Goettling's proceſs for preparing muriate of barytes in this way.

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* Van Mons's *Journal de Chimie*, Vol. VI. p. 92. Abridged from the *Taſchen-Buch fuer Scheidekuenſtler*.

† This muriate is obtained in abundance in our laboratories, as an adventitious product: it may be procured likewiſe at a very trifling expence by adding lime to the mother-water left after refining common ſalt.

Muriate of lime, how obtained.

mother-water of the firſt cryſtallization, which is almoſt wholly muriate of lime, is to be ſet aſide for a freſh operation, or for any other purpoſe.

The ſalt to be purified by re-cryſtallization.

As the inſoluble maſs of ſulphate of lime ſtill contains a large quantity of ſulphate of barytes, indeed about half the original quantity, it is to be treated afreſh as above, with one fourth part of muriate of lime; and the ſame proceſs is to be repeated with an eighth part of the ſame ſalt. Thus we obtain an addition from one to two eighth parts of muriate of barytes. The ſalts of the various cryſtallizations require to be diſſolved and re-cryſtallized anew, in order to free them from a little muriate of lime, which adheres to them in the firſt cryſtallization.

XXI.

Obſervations on the Rectification of Nitric Acid, by Mr. Steinacher, *Druggiſt at Paris.**

Nitric acid contaminated in its firſt portions with muriatic. Why, according to Berthollet.

True reaſon is the preſence of an exceſs of water.

IT has long been known, that the firſt portions of nitric acid diſtilled from litharge contain muriatic acid. Berthollet explains this phenomenon by ſaying, that the oxide of lead dividing its action between the two acids, both are ſubjected to the action of expanſibility produced by the heat. Meſſrs. Welter and Bonjour aſſert, that, if muriate of ſilver be employed, oxigenated muriatic acid is formed, which riſes with the firſt portions. If I may be allowed to give the reſults of my labours after thoſe of ſo many able chemiſts, I would ſay, that, when the nitric acid has been ſufficiently concentrated before being ſubjected to rectification on ſilver, or on oxide of lead, the firſt part of the rectified acid is found on trial to contain no muriatic acid, notwithſtanding the nitric acid contained much of it after its concentration; and hence I infer, that an exceſs of water is the true cauſe, that diminiſhes the attraction of the muriatic acid for the oxide of lead or of ſilver.

Concentrating the acid inſufficient, if the proportion of litharge be improper, and the diſtillation carried to dryneſs.

The operator however would in vain expect to ſucceed by merely concentrating his acid before rectifying it, if he uſed a determinate proportion of litharge, or diſtilled to dryneſs, as ſeveral authors direct. The quantity of litharge muſt vary from one to eight ſixteenths of the weight of the acid, according to its degree of impurity. On the other hand, if diſtilled to dryneſs, the latter portions of the nitric acid will carry over with them in ſolution muriate of lead, or of ſilver.

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* Van Mons's *Journal de Chimie*, Vol. VI. p. 88.

Mr. Steinacher's proceſs.

Four kilogrammes (10lb. 8oz. 16dwt. troy) of nitric acid of the ſhops, at 35°, containing muriatic acid, and a very little ſulphuric, are firſt diſtilled in a reverberatory furnace in a retort placed on an earthen veſſel filled with ſand. The fire muſt be ſo regulated that the drops ſucceed each other ſlowly, and half the acid is to be thus drawn off. It will then give 15° of Baumé's areometer. What remains in the retort is to be poured into a bottle. Its ſpecific gravity will be expreſſed by 4° of the areometer. Litharge being thrown into it in fine powder, and ſtirred with a glaſs rod, will be converted into a white powder in a few hours. More litharge is then to be added in the ſame manner; and this is to be continued, till the litharge retains its colour after ſeveral hours ſtanding. The muriate and ſulphate of lead are then to be left to ſubſide entirely, and the acid is to be decanted off into a tubulated glaſs retort, placed on a ſmall earthen plate filled with ſand; in the midſt of a reverberatory furnace, all the parts of which are retained except the dome. A receiver is to be adapted, which fits cloſely without luting; for, as the vapour of the acid eaſily deſtroys every kind of lute, the product would otherwiſe be liable to become impure; and the diſtillation is to be ſo conducted, as to admit a ſhort interval between the fall of each drop. Great care muſt be taken not to ſuffer the acid to boil, for thus it would be diſſipated in incompreſſible vapour. The firſt half that comes over marks 35°, the ſecond 40°. Both portions are colourleſs, and have all the properties of a very pure nitric acid, if $\frac{1}{16}$ of the liquor be left in the retort.

Beautiful cryſtals of muriate of lead may be obtained when half the acid is diſtilled off.

If a ſtop be put to the diſtillation after the firſt portion is ſeparated, and the retort left to grow cold, you will obtain a beautiful cryſtallization of muriate of lead in large and very brilliant ſtriated hexaedral laminæ. This ſalt is a true muriate, for ſulphuric acid expels from it vapours eaſily diſtinguiſhable to be thoſe of the muriatic acid. On continuing the diſtillation, theſe cryſtals gradually loſe their regular figure, and at length fall to the bottom in a powdery precipitate.

XXII.

Eaſy Method of making the very combuſtible Oxide of Phoſphorus.

By Amicus.

To Mr. NICHOLSON,

Dear Sir,

Oxide of phoſphorus.

YOU know very well that phoſphorus united to a much ſmaller proportion of oxigen than is requiſite to render it into the acid ſtate, brings it into the condition of an oxide which ſets on fire ſulphur, on juſt rubbing it againſt a common match. But the common method of oxidizing phoſphorus for phoſphoric matches by fixing it in the bottles with a hot iron, is troubleſome and waſteful. This oxidation, however, may be effected with great facility and economy by expoſing a large proportion, viz. a hundred grains of phoſphorus in a jar containing half a pint meaſure of oxy-muriatic acid, in which circumſtance the phoſphorus will be melted and fume, but ſcarcely take fire. After cooling, it muſt be kept excluded from the air, to prevent the inflammation from mere expoſure.

Dear Sir, Yours,

AMICUS.

May 28*th*, 1805.

XXIII.

Deſcription of an extremely ſenſible Micro-electrometer. By Mr. Marechaux.[†]

A piece of leaf ſilver is ſuſpended ſo as to be moveable in a glaſs cylinder.

IN a glaſs cylinder, about an inch and half in diameter and five or ſix inches high; a piece of leaf ſilver is ſuſpended from a ſmall pair of nippers, capable of being lowered or elevated as the length of the leaf may require. The piece that carries the nippers may likewiſe be moved horizontally, ſo that the leaf may be moved at pleaſure nearer to or further from a ſphere of copper, which is one of the poles of the inſtrument.

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[*] Tranſlated from Von Mons's *Journal de Chimie*, Vol. VI. p. 38. Abridged by Van Mons from Gilbert's *Annalen der Phyſik.*

The

Through a hole in the cylinder a micrometer-ſcrew paſſes,

The glaſs cylinder has, about 1″ (centimetre, near 4 lines Engliſh meaſure?) from the plate on which it is fixed, a ſmall hole, through which paſſes the extremity of a micrometer-ſcrew, about the ſize of a large gooſe-quill, and very carefully cut. This ſcrew has fifty threads in a Rhynland inch, cut very deep, though very fine. It is made of two pieces, and ſhould be at leaſt three quarters of an inch long, to avoid any ſhake. The extremity of this ſcrew carries a little ball, which is put on after the ſcrew is paſſed through the opening in the cylinder. To avoid all friction againſt the glaſs, care is taken that the ſcrew, when turned, does not touch the edges of the cylinder. The ſcrew carries a plate three inches and a half (3.8 Engliſh) in diameter, which has 360 diviſions, and conſequently divides each thread of the ſcrew into as many parts.

by which the diſtance of $\frac{1}{18000}$ of an inch may be meaſured.

In this manner we are enabled to determine the ſphere of activity of the two electricities in 18000ths of a Rhynland inch. The mounting which contains the female ſcrew has a ſmall pillar, which advances on the plate, and carries an index, by which the degrees are marked with preciſion.

Mode of preparing the inſtrument for uſe.

To uſe this inſtrument, which is perfectly inſulated by the glaſs plate on which it reſts, the firſt thing is to place the plate in ſuch a poſition that the 0 ſhall be exactly under the index. The adjuſting ſcrew which carries the nippers is then to be moved till the leaf ſilver is ſo near the ball, that no light paſſes between them. Thus we have the point of contact, and of 0 for the ſphere of activity of the two electricities. To be certain that the leaf is brought as cloſe as poſſible to the ball without being forced out of the vertical direction, the micrometer ſcrew ſhould be moved a turn firſt backward then forward ſeveral times, and the poſition of the leaf obſerved every time the ball is brought into contact with it. The inſtrument being thus adjuſted, the micrometer-ſcrew muſt be moved backward one turn, and then we have between the leaf and the ball a diſtance of one-fiftieth of an inch, which may be ſubdivided at pleaſure by means of the plate; for with a plate near four inches in diameter, and by means of the fine needle on which it turns, we may diſtinguiſh half or even a quarter of a degree if neceſſary.

Apparatus for aſcertaining the perpendicularity of the leaf.

An adjuſtment fixed to the plate on which the cylinder reſts ſerves to aſcertain whether the leaf of ſilver be in fact drawn out of the vertical line by any attractive power. This conſiſts of a fine ſilk thread, ſtretched in the ſame plane as the ſilver leaf. By means of the ſcrew this thread may be moved both horizontally and vertically, ſo as to follow the movements of the ſilver leaf.

Mode of uſing the inſtrument.

Every thing being thus arranged, make a communication by means of conducting wires between a ſingle couple of metallic diſks, or one conſtituent part of a pile, placed on a plate of glaſs, and the inſtrument, ſo that one of the metals ſhall communicate with the top of the inſtrument, and the other with the bottom. Then by means of a glaſs handle fitted to it move the plate ſlowly from one degree to another, and you will find the leaf touch the ball with ordinary electricity, when it is 60° or 80° of the micrometer ſcrew from the vertical plane, in which the leaf ſilver reſted before its communication with the metallic diſks. This diſtance increaſes for every pair of diſks added; and as the ball remains fixed at the point to which the ſcrew has carried it, the motion of the inſtrument may be obſerved with great accuracy.

Its extreme ſenſibility.

This inſtrument is ſo ſenſible, that, if a ſlender glaſs tube be rubbed but twice, and brought near the apparatus, though ſeveral inches from its ſummit, it paſſes through the whole extent of its ſcale. It is for this reaſon the inventor calls it a *micro-electrometer*, becauſe we can meaſure only very weak degrees of electricity with it.

XXIV.

Action of Phoſphorus on the Solutions of Metals. By Mr. SCHNAUBERT*.

Phoſphorus obſerved to precipitate metals, by Sage, Ilſemann, and Mrs. Fulhame.

SAGE† had already obſerved, that phoſphorus precipitates the ſulphates of copper and of mangaheſe. After him Ilſemann‡ obtained a cryſtallization of ſilver in the humid way by means of phophorus. Still more recently Mrs. Fulhame* publiſhed experiments on the reduction of ſome metals by phoſphorus diſſolved in ether. Laſtly, Mr. Schnaubert has undertaken a new inveſtigation of the ſubject, the principal reſults of which are as follows:

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* Van Mons's *Journal de Chimie*, Vol. VI. p. 95. Abridged from Goettling's *Chemiſches Taſchenbuch*.

† *Analyſe chimique et Concordance des trois Régnes.*

‡ Crell's *Chemiſche Annalen*, 1789, Tom. II. p. 323.

Schnaubert's experiments.

Gold.

Two little bits of phoſphorus were put into a nitro-muriatic ſolution of gold diluted with a ſmall portion of water. At the expiration of twenty-four hours the ſolution was completely colourleſs, and pellicles of the colour of metallic gold ſwam on the ſurface of the liquid. The phoſphorus itſelf was covered with a deep brown coating, and in this was obſervable in ſeveral places thin layers of reduced gold. At the place where the phoſphorus was a black circle was perceived. The ſolution thus treated by phoſphorus, had not a ſingle atom of gold left in it.

Gold completely precipitated in a metallic form by phoſphorus.

Silver.

Some phoſphorus, which was left for twenty-four hours in a nitric ſolution of ſilver diluted by water, was completely covered with metallic ſilver in the form of dendrites, the ramifications of which were directed upwards. During ebullition this remarkable cryſtallization of ſilver, which made the phoſphorus appear as if garniſhed with points, aſſumed firſt a white colour, and afterward formed a light black maſs, which at length became of a light brown colour.

Silver precipitated in dendritic cryſtals,

which boiling converted into a phoſphure.

Quickſilver.

Mercury diſſolved in nitric acid is precipitated on the phoſphorus in the form of little metallic globules, which cover it entirely. By heating to ebullition the mercurial globules diſappear, and a black maſs without any metallic luſtre is formed.

Mercury precipitated in globules,

which heat converts to a phoſphure.

Lead.

At the ordinary temperature phoſphorus did not act on the nitric ſolution of lead, though the digeſtion was continued for ſeveral days: at a boiling heat however a change was obſerved in the phoſphorus, which was covered with a grey colour ſlightly metallic.

Lead not reduced without a boiling heat.

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* Eſſay on Combuſtion, &c.

Copper.

Copper powerfully acted on.

No metallic ſolution was more ſtrongly attacked by phoſphorus than that of copper in nitric acid. The phoſphorus was no ſooner introduced into the ſolution of copper, than it aſſumed a black colour. In twenty-four hours it was covered with a ſtratum of metallic copper in very thin layers *; and the ſolution had become much paler. The application of heat cauſed drops of phoſphorus to flow out upon the reduced copper, where they immediately aſſumed a black colour. Theſe drops after a time were in their turn covered with a metallic coat of copper. After this the ſolution was perfectly colourleſs, and ammonia did not detect in it the leaſt particle of metal.

Completely precipitated from the ſolution.

Tin.

Tin partly reduced, partly converted into phoſphure.

Several bits of phoſphorus were put into a ſolution of tin in nitro muriatic acid. The next day the phoſphorus was coloured of a deep brown, only in ſome parts a metallic colour was obſervable. Theſe metallic ſpots diſappeared on boiling, and the phoſphorus became ſtill deeper coloured.

Sulphate of copper forms a beautiful experiment.

The phoſphorus enveloped in a caſe of particularly malleable copper impervious to air.

* In making this experiment with a ſolution of ſulphate of copper, and ſlightly heating the mixture, at firſt a vapour ariſes, conſiſting of phoſphorus gas, that carries off with it ſome ſmall particles of phoſphorus, which take fire on the ſurface of the ſolution: but the extrication of this vapour ſoon ceaſes, and the phoſphorus becomes hermetically encloſed in a box of copper, in which it is defended againſt any farther action of the ſulphate, and even of the air, to whatever temperature ſhort of fuſing the copper it be afterwards expoſed. The plate of copper that forms this covering is two or three lines thick: it poſſeſſes more tenacity than common copper, for it may be flattened with a hammer in different directions without cracking, which at the ſame time proves the great compreſſibility of the phoſphorus; and it ſhines with a very pure metallic luſtre. On opening the box carefully with a cutting inſtrument, the phoſphorus is found in it retaining perfectly its form, filling its copper caſe completely, and not appearing even to have acted upon the ſulphate.

Other metals did not produce this effect.

I did not obtain the ſame effect with ſeveral other metals which I tried, no doubt on account of their containing more oxigen.

VAN MONS.

Manganeſe.

Manganeſe reduced by it.

Some bits of phoſphorus were put into a ſulphuric ſolution of manganeſe. The next day the phoſphorus was of a deep brown colour, and on its ſurface was perceivable a pleaſing mixture of colours, owing to the reduced metal. The mixture was then heated to ebullition, and after it had grown cold, the manganeſe was found reduced in the form of little radiating lines on the fuſed phoſphorus, intermixed with a few ſmall globules of the white colour of tin.

General concluſions.

In theſe experiments we may obſerve, that the phoſphorus, beſide deoxidating the metals, united with the metals when reduced to form phoſphures, as was evident in the ſolutions of ſilver, mercury, and tin.

XXV.

Account of a new Pyrometer, which is capable of indicating Degrees of Heat of a Furnace. By Mr. J. G. SCHMIDT, *of Yaſſy, in Moldavia. From the Author.*

Pyrometrical ſubſtances.

ITHOUT entering into any detail concerning the ſubſtances beſt calculated for pyrometrical enquiries, I flatter myſelf that it will be admitted that thoſe muſt receive the preference which are capable of regularly contracting or expanding, without altering their chemical properties, when ſubjected to elevated temperatures.

Gaſes are the beſt for ſtrong heat.

The permanently elaſtic aeriform fluids appear to me to be ſuperior in thoſe reſpects to any other bodies.

Atmoſpheric air in a veſſel of platina.

Let atmoſpheric air be freed from moiſture by cauſtic alkalis, or other bodies, and included in a veſſel of platina. This veſſel A (*Fig.* 3, *Pl. VI.*), which may be made of any convenient ſize, is connected with the tube B B, of as fine a bore as poſſible. This tube is alſo made of platina, and reaches into a veſſel C, which is filled with water up to *e e*, and into this the tube is fixed air-tight. Out of the veſſel C riſes a glaſs cylinder G hermetically ſealed, including a thermometer, and a graduated tube F F is ſecured into the veſſel C in a ſimilar manner.

Uſe and application.

This is the whole conſtruction of my pyrometer. To make uſe of it nothing more is neceſſary than to introduce the platina veſſel A into the furnace the temperature of which is to be learned. The moment the included air is acted upon by the heat it expands, and expels the water up into the graduated tube F F. This riſe will take place accordingly as heat increaſes. If care be taken that the air be cooled in the veſſel C as much as poſſible (which will be the caſe from the large ſurface of water to which it is expoſed), it is obvious that a volume of water equal to the volume of air in the veſſels of platina, can never paſs up into the tube. The refrigeration may be facilitated by the application of vaporizable fluids, ſuch as ether, alcohol, &c.

If the degree of temperature be obtained which the air had before it was ſubjected to the experiment, and a proper allowance be made for the preſſure of the water in F F, the true expanſion of the air may thus be found, and compared with the reſpective temperatures.

XXVI.

New, eaſy, and economical Method of ſeparating Copper from Silver. By Mr. GOETTLING[*].

Sulphuric acid uſed inſtead of nitric on account of cheapneſs,

THERE are four methods of ſeparating copper from ſilver, all of which require the alloy to be diſſolved in nitric acid. As this acid is very dear, Mr. Goettling thought of uſing the ſulphuric in its ſtead, which is comparatively very cheap. His ſucceſs perfectly equalled his expectation, and the following is his method:

and with perfect ſucceſs.

Deſcription of the proceſs.

Having aſcertained by the touchſtone, or in any other way, the proportion of ſilver contained in the alloy, take one part of ſulphuric acid for every part of ſilver, and for every part of copper three parts and three-fifths of a part of the ſame acid. Dilute the acid with half its weight of water, and pour it into a matraſs on the alloy reduced to very ſmall pieces. In order to promote the action of the acid, it is of uſe to put one part more to every ſixteen parts of the alloy. The matraſs is then to be placed in a ſand-heat, and the acid brought to a ſtate of ebullition. In two or three hours time the alloy is commonly diſunited and converted into ſulphate, particularly if care be taken to ſtir the maſs from time to time with a glaſs ſpatula. This maſs is thick, and frequently hard. While it is ſtill hot, ſix or eight times its weight of boiling water is to be added to it, and it is to be left ſome time longer on the fire. The ſulphate of copper will be diſſolved, and great part of the ſulphate of ſilver will be precipitated. The operator will now examine whether the whole be completely diſſolved; and if it be, a plate of copper, or ſome pieces of copper or halfpence tied up looſely in a piece of coarſe linen, muſt be ſuſpended in the mixture, and the whole kept boiling for ſome hours. The ſulphate of ſilver will thus be decompoſed, and the ſilver ſeparated in the metallic ſtate.

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[*] Tranſlated from Van Mons's *Journal de Chimie*, Vol. VI. p. 77. Originally publiſhed in the *Taſchen-Buch fuer Scheidekuenſtler*, and abridged by Van Mons.

Mode of aſcertaining whether the ſeparation be complete.

To aſcertain whether the ſeparation be complete, a few drops of ſolution of muriate of ſoda are to be dropped into a little of the liquor. If a cheeſelike precipitate be formed, it is a proof, that all the ſilver is not ſeparated, and in this caſe the ebullition with the copper muſt be continued longer. After the whole of the ſilver is ſeparated, the liquor is to be poured off, the precipitated ſilver is to be well waſhed, and the entire ſeparation of the cupreous ſalt is to be aſcertained by the addition of a few drops of liquid ammonia to the water with which the precipitate has been waſhed, which, if it contain any copper, will be rendered blue by the ammonia. After the ſilver is thoroughly freed from the ſulphate of copper, it may be kept in the ſtate of powder as it is, or it may be fuſed with a fourth or at moſt half its weight of ſulphate of potaſh.

May be kept in powder, or fuſed with ſulphate of potaſh.

The water poured off is then to be mixed with what was uſed for waſhing the precipitate, and evaporated in a copper pan, ſo as to obtain the ſulphate of copper by cryſtalliſation. The blue vitriol thus produced will be at leaſt equal in value to the ſulphuric acid employed.

Blue vitriol obtained equal to the coſt of the acid.

If any parts of the alloy remained undiſſolved, it ſhould be ſeparated by decantation and reſerved for a future operation.

Undiſſolved alloy to be ſet by.

SCIENTIFIC NEWS.

Extract of a Letter from BRUGNATELLI, *concerning the non-existence of the charged Pile**.

VOLTA has made many experiments on piles compoſed of a ſingle metal, and a ſingle wet ſtratum, which, from being inactive by themſelves, become more or leſs active after affording a paſſage for a longer or ſhorter ſpace of time to an electric current ſet in motion by active pile, &c.

Ritter aſſerted that a pile compoſed of one fluid, and one metal, was capable of being charged by another pile, but the fact is, that the fluid is converted into two different fluids by the electric current.

Ritter, the moſt judicious of the galvanic philoſophers of Germany, has aſſerted, as Volta ſays, that the active pile, or common electrometer, tranſmits a real charge to the pile that is itſelf inactive, which it therefore calls the *charged pile*. Volta however has convinced himſelf, that no charge is tranſmitted, but, by virtue of the ordinary chemical action, the electric current being continued, changes the ſingle wet ſtratum interpoſed between two pieces of gold, for example, into two different fluids, one acid, by which the electric current iſſues out of the metal, and the other alcaline, by which it enters; which conſtitutes a pile of the ſecond order, namely, of one metal and two fluids of different natures, the action of which however does not continue long, becauſe the fluids ſoon mix.

Mechanical work by Mr. Gregory.

Mr. GREGORY of the Royal Military Academy, Woolwich, has now in the preſs a Treatiſe on Mechanics, which is intended to be publiſhed in two volumes octavo. The firſt will be devoted chiefly to the theory, and will be divided into five books under the ſeveral heads of ſtatics, dynamics, hydroſtatics, hydrodynamics, and pneumatics. The ſecond volume will be chiefly appropriated to practical and deſcriptive ſubjects, and will commence with general remarks, rules, and tables, relative to the nature, conſtruction, and ſimplification of machinery; the effects of friction, and the rigidity of cords; and eſtimates of the varied energy of different firſt movers, &c. Theſe will be followed by deſcriptions arranged alphabetically, of about 100 of the moſt curious, uſeful, and important machines. In this latter part, Mr. Gregory has been promiſed communications from ſome celebrated civil engineers, ſo that he hopes, on the whole, to render the work in ſome meaſure deſerving the attention of thoſe who are engaged in the cultivation and improvement either of the theory or the practice of mechanics.

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* Van Mons *Journal de Chimie*, Vol. VI. p. 132.

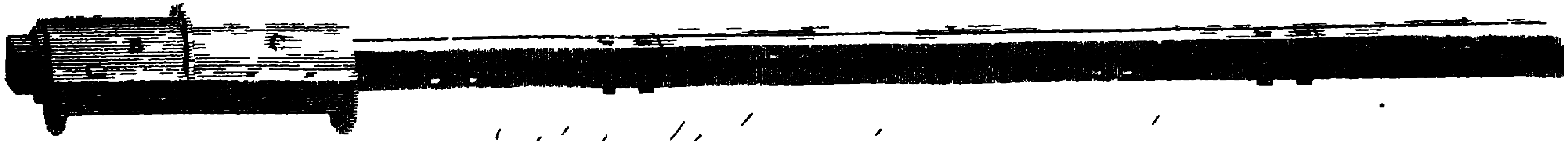
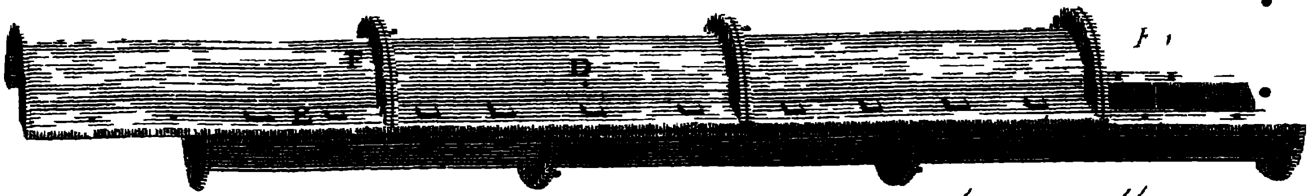


Fig. 1



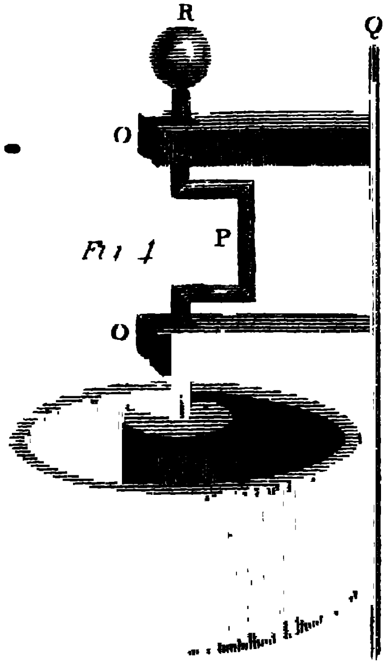
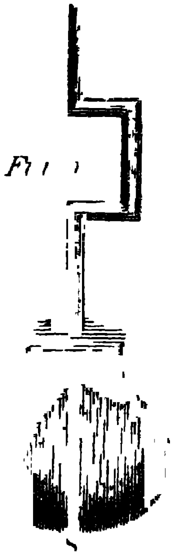


Fig. 4



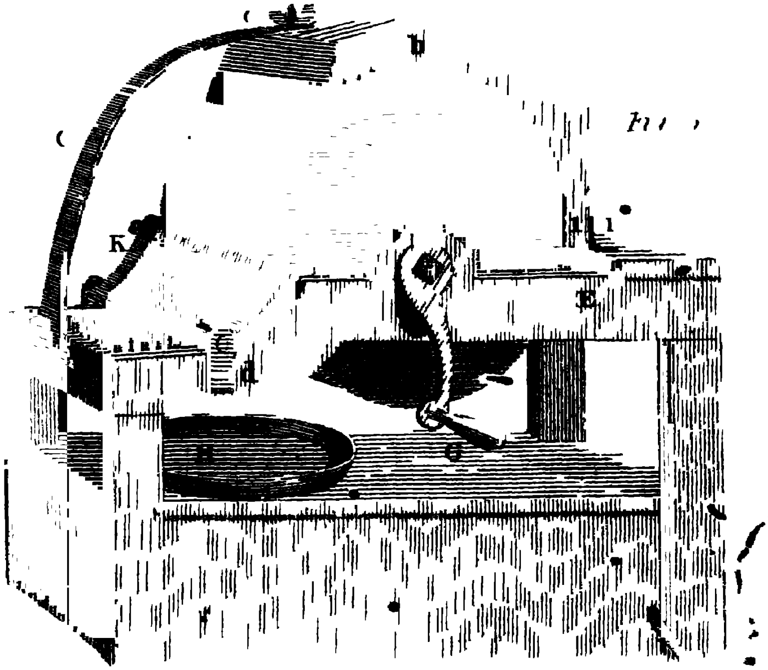
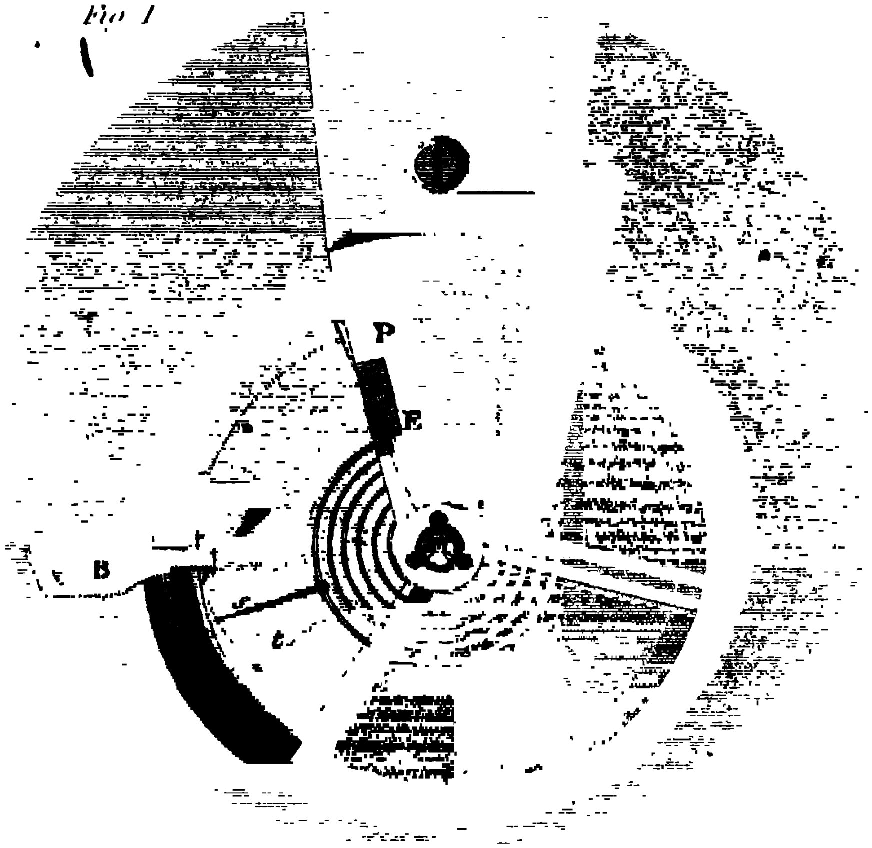
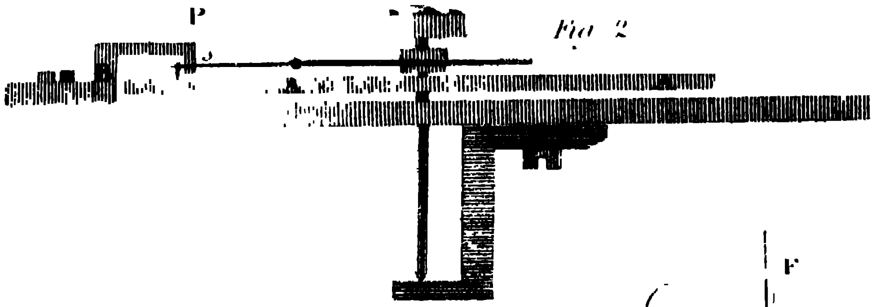


Fig. 1



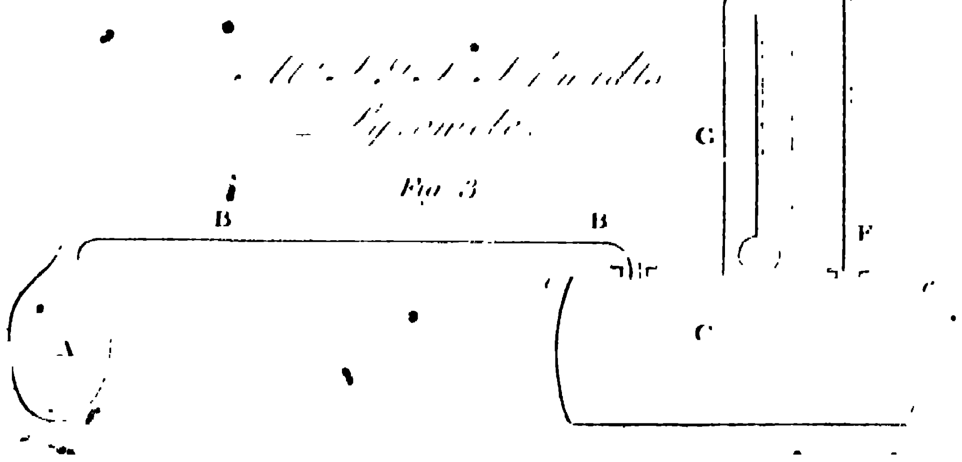
[illegible] method of hanging the balance of a time keeper.

Fig. 2



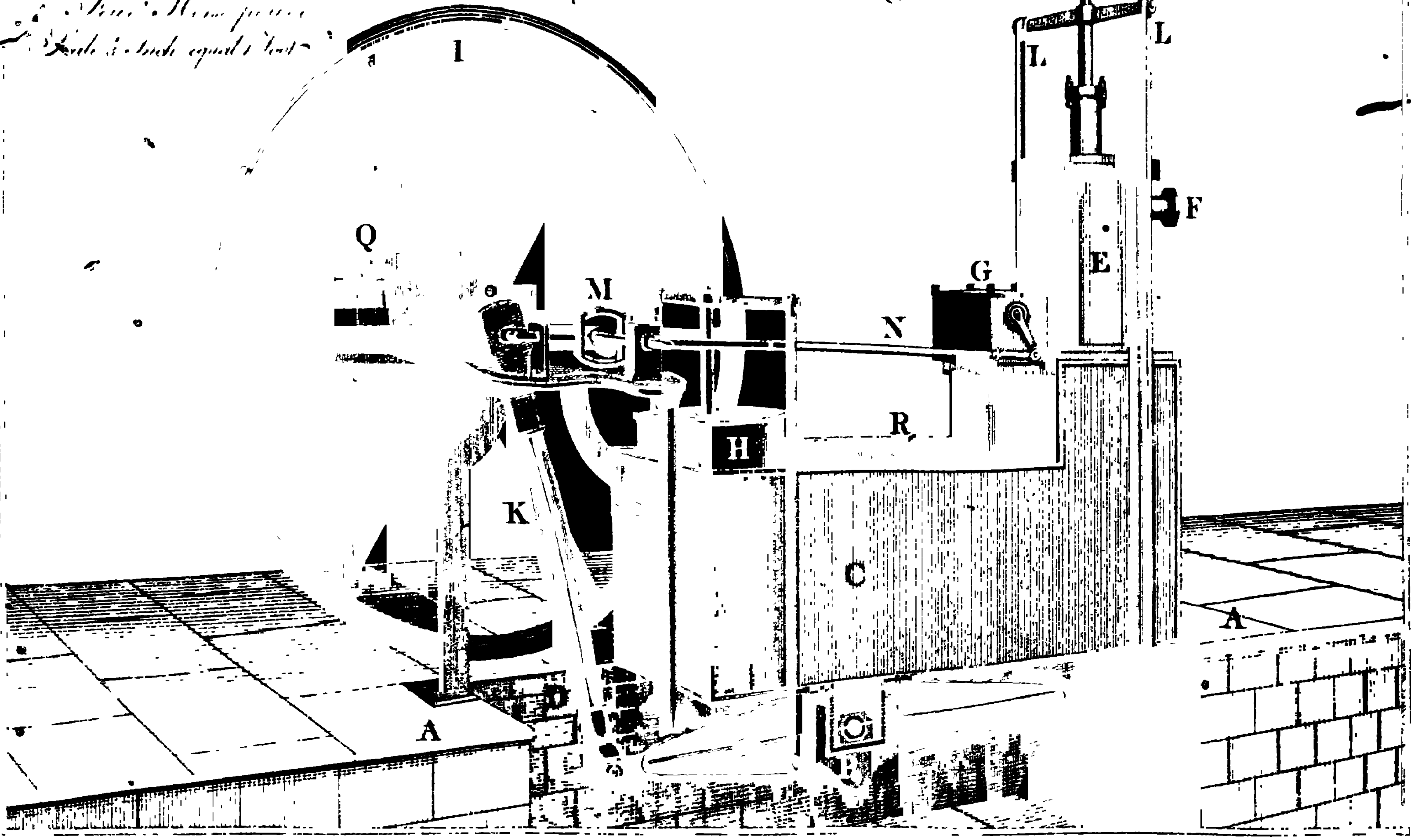
[illegible] Pyrometer.

Fig. 3



Mr Murrays Portable Steam Engine.

Four Horse power
Scale 1 Inch equal 1 Foot



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1805.

ARTICLE I.

Remarks on the Eſtimation of the Strength of Horſes. In a Letter from Mr. O. GREGORY, *of the Royal Military Academy, Woolwich.*

To Mr. NICHOLSON.

SIR,

THE remarks of your ingenious correſpondent, Mr. Hornblower, on the various eſtimates of the *Power of a Horſe*, and the abſurdity of adopting a quantity ſo fluctuating and ſo difficult to aſcertain, as a common meaſure by which the powers and effects of ſteam engines and other machines are to be eſtimated and compared, have induced me to throw together a few obſervations on the ſame ſubjects; the theoretic part of which, though familiar to moſt men of ſcience, ſeems not to be always known, or at leaſt recollected, by ſome perſons who are employed in the practice; and which are altogether much at your ſervice for inſertion in the Journal, if you think them likely to be of any utility.

Reference to Mr. Hornblower's paper on horſe powers.

Dr. Deſaguliers has given another eſtimate of the labour of a horſe, beſide that mentioned by Mr. Hornblower, and which indeed does not ſeem very conſiſtent with it; for in vol. II. p. 251. of his Experimental Philoſophy, he affirms that a horſe in

Eſtimate of horſe power by Deſaguliers. 200lb eight hours per day, at 2½ miles per hour; or 3⅔ feet per ſecond,

—by Sauveur 189lb. avoird. three feet per ſecond,

—by the author 130lb. three feet per ſecond.

an advantageous ſituation, is able to draw 200lbs. eight hours a day, walking at the rate of 2½ miles in an hour, or 3⅔ feet in a ſecond. This ſtatement of the power of a horſe, though it is not ſo great as that which is arbitrarily aſſumed by Meſſrs. Boulton and Watt, exceeds the determination of M. Sauveur, who eſtimates the mean effort of a horſe at 175 French, or 189 avoirdupois lbs. with a velocity of rather more than three feet per ſecond; and it probably exceeds Mr. Smeaton's ſtatement of 550lbs. moved 40 feet in a minute; though, as will ſoon be ſeen, we are not furniſhed with proper data to inſtitute a compariſon between theſe various reſults. It is probable, however, as obſerved by the ingenious contributor of the article at page 216, vol. IX. of your Journal, that "the loweſt of theſe "performances is more than equal to the average power of a "horſe employed in huſbandry for eight hours per day." So far as my own obſervations on this point extend, I am inclined to conclude that the average work of a ſtout London cart horſe, for eight hours in a day, is little if any more than 130lbs. moved at the rate of three feet in a ſecond, or 2$\frac{1}{22}$ miles per hour. But this it would be ridiculous to aſſume poſitively as a univerſal unit of meaſure, in a caſe where the cauſes of variety are ſo numerous, and my opportunities of experiment comparatively few. The eſtimate juſt given, it ſhould be obſerved, is not intended to expreſs what a horſe can draw upon a wheel carriage, where friction alone is to be overcome, after the load is once put into motion, and where a horſe will often draw much more than 1000lbs. but the weight which a horſe would raiſe out of a well, &c. the animal acting by a horizontal line of traction turned into the vertical direction by a ſimple pully or roller, whoſe friction is reduced as much as poſſible.*

Miſapplication of the general principle that gain in power i loſs in time.

Before we can inſtitute any compariſon between the reſults of different experiments, it will always be neceſſary to enquire what machine was interpoſed between the weight moved and the animal, in each caſe, that we may thence deduce the real velocity with which the animal moved, from the velocity of the weight or load given by the obſervations. This is too frequently omitted in conſequence of an implicit reliance upon a maxim, which, though highly uſeful under proper reſtrictions, is far from univerſal in its application. In the caſe before us, if we admit the maxim now alluded to, namely, that *what is gained in power is loſt in time,* with regard to the machine through whoſe intervention the velocity of the weight is rendered different from that of the horſe, it would be unſafe to adopt it in the appreciation of the varied energy of the animal when moving with different velocities. The reaſon of this is obvious. The energy of the horſe is obliged to be employed not only in overcoming the weight or reſiſtance which oppoſes his progreſs, but in part in moving *himſelf;* for the particles which conſtitute his frame poſſeſs weight and inertia, and therefore cannot be put into motion without effort. Hence it follows that there is a certain velocity, which may be denoted by U, with which, when the animal moves, his whole power will be employed in producing his own motion ſolely, without being able to move any other body. If a body whoſe maſs is M be attached to the horſe, ſo that he cannot move without giving an equal velocity to the extraneous body, the ſame effort being employed both in moving the animal and the maſs attached; the velocity V, with which they move muſt neceſſarily be leſs. And if M be farther increaſed while the weight and energy of the horſe continue the ſame, the velocity V will be ſtill farther diminiſhed; and thus as M increaſes V will diminiſh, until when M arrives at a certain magnitude, W, the animal is unable to make any progreſſive motion, and exerts his force at what is called a dead pull. If M exceeds W, then will V become negative, and inſtead of the animal advancing with the load, the load will compel him to move backwards, and no uſeful work can be accompliſhed.

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* The late Mr. More, Sec. to the Society of Arts, found by the interpoſition of a graduated ſpring inſtrument between the horſe and his work, that the re-action was between 70 and 80lb. when the velocity was three miles in an hour. I think the work was ploughing. See Philoſ. Journal, quarto, Vol. III. 136. - N.

An horſe, having himſelf to carry, does not exert his force againſt his work only.

Now theſe circumſtances may be expreſſed algebraically, by the general formula $M \propto (U-V)^n$, in which the exponent n can only be determined by means of judicious and numerous experiments, where the magnitude of M ſhould be aſcertained for many variable values of V between the terms $V = U$, and $V = 0$. From this theorem, following the common rules for the maxima and minima of quantities, it may readily be found that in order to have the *uſeful* work done the greateſt poſſible, we muſt increaſe or decreaſe the weight till V becomes $= \frac{U}{n+1}$

Method of computing the effect of horſes.

when the performance will be denoted by $(n+1)^{\frac{\;}{n+1}} \times WU$,

And if the value of V thus exhibited be once aſcertained experimentally, we need never be apprehenſive of a material loſs by a ſmall variation from it; for by a well known property of thoſe quantities which admit of a proper maximum and minimum, a value aſſumed at a moderate diſtance from either of theſe extremes will produce no ſenſible change in the effect.

Example.

In ſome of the actions of men, ſuch as dragging a boat along a canal, &c. the value of n in the preceding theorems has been found to be nearly $= 2$. And the draught of horſes is conformable to a law not widely different. The beſt experiments which have yet been made on this point with regard to horſes drawing in nearly rectilinear paths, lead us to conclude that n is then very nearly $= \frac{9}{4}$, in the expreſſion $M \propto (U-V)^{n}$. Aſſuming therefore for the utmoſt walking velocity of a horſe, the value $U = 9$ feet per ſecond, a value which is quite high enough, any propoſed eſtimates of the ſtrength of this animal may be compared with facility. Thus, for example, let us enquire which is greater, the eſtimate of Mr. More (mentioned by Mr. Hornblower) of 80lbs. three miles per hour, or $4\frac{2}{5}$ feet per ſecond; or that of 130lbs. moved at the rate of three feet per ſecond? Here we ſhall have

$$(9-3)^{\frac{9}{4}} : (9-4{\cdot}4)^{\frac{9}{4}} :: 130 : 71\tfrac{1}{2}\text{lbs. nearly.}$$

The operation may eaſily be performed by means of a table of logarithms, and ſhews that the mean eſtimate I have laid down, when reduced to the ſame velocity as that by Mr. Moore, furniſhes a reſult leſs than his by $8\frac{1}{2}$lbs. Which of theſe is the moſt accurate can only be determined by future experiments.

On the power of horſes walking in circular paths;

If, however, either of theſe eſtimates ſhould be adopted, it may be proper to remark that they would not hold with regard to the power of horſes working in circular paths; yet, if it be at all proper to uſe *horſe powers* in eſtimating the energy of machines, it ſeems moſt natural to take theſe powers as exerted by the animal in a round walk; ſo that it is ſtill neceſſary to have a ſeries of experiments to determine the values of n, and the relation of M and $V_{}$ when horſes draw in circular walks of different radii. I ſay, of *different* radii, becauſe it is certain that *cæteris paribus*, the greater the radius of the circle

—of different radii.

in whoſe circumference the animal moves, the leſs fatigue will be occaſioned by that kind of motion. Indeed it is obvious, that ſince a rectilinear motion is the moſt eaſy and natural for the horſe, the leſs the line in which he moves is curved, with the greater facility he will walk over it, and the leſs he need recline from the vertical poſition. Beſides this, with equal velocity at the circumference, the centrifugal force will be leſs in the greater circle, which will proportionally diminiſh the friction of the cylindrical part of the trunnions, and the labour of moving the machine. And farther, the greater the radius of the horſe-walk, the nearer the chord of the circle in which the horſe draws is to coincidence with the tangent, which is the moſt advantageous poſition of the line of traction. Hence it follows, that although a horſe *may* draw in a walk of 18 feet diameter, yet he will work with far greater eaſe in one whoſe diameter is 35 or 40 feet; and it is very deſireable that an experimental enquiry ſhould be made to aſcertain the proportion and abſolute quantity of work in different circles.

The larger walks are moſt advantageous.

Deſcription of a mechanical apparatus for meaſuring the re-action in mills.

I am of opinion that it would not be difficult to make ſome uſeful experiments, while work was actually carrying on at any horſe mill, or machine where horſes are conſtrained to move in a circular walk. The ſimple drawing which accompanies this letter will aſſiſt in conveying a clear idea of the method which I fancy might be advantageouſly adopted. Let AB *Fig.* 1. *Plate* X. be the vertical ſhaft to which the horizontal horſe poles AC, AD, are attached. Let one horſe work the machine by drawing at the ear E; but, inſtead of the tranſverſe bar to which the harneſs is fixed being ſimply hung upon the hook *h*, let a good ſpring ſteelyard be interpoſed between that croſs-bar and the hook, the graduations of which ſhall, when the machinery is put into motion, indicate the reſiſtance (in lbs.) overcome by the animal, including the weight of the maſs moved, the friction, &c. Near the extremity of the oppoſite horſe-pole AD, let there be fixed a ſtrong and correct common ſteelyard, whoſe diviſions ſhall ſhew the various weights from 40 or 50 to 200lbs. and whoſe centre of motion ſhall be at the point *f* on the fixed ſtand. Let the cord *c* which is faſtened to the ſhorter arm of this ſteelyard, paſs (with as little friction as poſſible) over the pulley *p*, and thus, being turned into the horizontal direction, or rather, inclining a little upwards, let it be fixed to the croſs bar

Deſcription of a mechanical apparatus for meaſuring the re-action in mills.

bar of the harneſs of a ſecond horſe, equal in point of ſtrength to the former. Then if the two horſes, thus attached to the ears E and F, be made to paſs over the walk in the ſame direction, following each other conſtantly at the diſtance of a ſemi-circumference; while that which draws at the ear E overcomes the whole preſſure and reſiſtance oppoſed by the work, the other which draws at F by the cord over the pulley *p*, will raiſe the weight *w* of the ſteelyard; which therefore, by being moved to and fro upon the arm *f i*, may be brought to exhibit an exact counterpoiſe, or meaſure of the exertion and power of the horſe. And in order to enſure the greateſt degree of accuracy in this reſpect, the motion of the two animals, and the poſition of the weight *w*, ſhould be ſo adjuſted, that the ſame weight ſhould be ſhewn by the graduations both of the ſpring and of the lever ſteelyard. The ſhaking of the machinery will in ſome meaſure diſturb the effect; but an ingenious manager of the experiments will find means of checking this: and as to the centrifugal force to which the weight *w* is expoſed, it will never be of any material conſequence in any of the ſlow motions which will be produced by this kind of work.

Each experiment ſhould occupy the ſpace of a fair day's work for the horſes: for the concluſions deduced from ſhorter and irregular efforts are always erroneous in exceſs, and ſhould be guarded againſt. The rate at which the animals move may readily be aſcertained from the known circumference of the walk, and the number of rounds they are obſerved to make in 10 or 15 minutes. Thus, by continuing the experiments day after day, varying the velocity of the motion in ſome caſes, and the radius of walk in others, ſuch a ſeries of reſults might at length be obtained, as would in a great meaſure remove the obſcurity and doubt in which this buſineſs is at preſent enveloped. It is ſcarcely neceſſary to ſuggeſt the propriety of making a few experiments with a view of determining how far a load upon the back of each draught horſe, would aſſiſt him in his labour. Nor can it be requiſite to point out in what way, by means of ſuch ſteelyards properly applied to waggons, &c. upon tolerably ſmooth roads, and two horſes marching abreaſt, (one drawing the load, the other raiſing the weight,) experiments might be inſtituted to aſcertain the magnitude of the efforts of horſes when drawing in rectilinear paths. Judicious experiments having theſe purpoſes in view, would certainly be beneficial, as they would enable us to tell what advantages might be expected from the labours of this uſeful quadruped in different circumſtances. But with reſpect to the adoption of "*horſe power*" as a unit of force in eſtimating the power of ſteam engines, &c. I confeſs that if it were as well known, and as unvariable as the length of the day of the equator, I ſhould feel an averſion to applying it to any ſuch purpoſe. It is a common meaſure arbitrarily adopted, which has no neceſſary connection with the ſubject that is referred to it, which does not in any reſpect facilitate the computation of the powers of an engine, and which may, without proper caution, lead to conſiderable errors in the concluſions deduced from it.

The adoption of an arbitrary unit from the power of an horſe.

Before I cloſe this letter, already perhaps too long, I beg permiſſion to ſay a few words reſpecting the meaſure which is generally employed to determine the mechanical effect produced. This is the meaſure of the deſervedly celebrated Mr. John Smeaton, who ſays that "the weight of a body multiplied by the height through which it deſcends, while driving a machine, is the only proper meaſure of the power expended; and that the weight multiplied by the height through which it is uniformly raiſed is the only proper meaſure of the effect produced." Mr. Smeaton was led to the uſe of this meaſure by his profeſſional habits; and many who in this reſpect pay too great a deference to his authority, have adopted this meaſure as univerſal and preferable to any other. Taking this as a popular meaſure eaſy to recollect, and ſimple in its application, it undoubtedly has its uſes; but in many inſtances it is inadequate to the purpoſe for which it is propoſed. The late Profeſſor Robiſon has ſome excellent obſervations on this ſubject, in the article *Machinery, Sup. Encyclopædia Britan.* where he lays down the juſt meaſure to which the ſcientific inveſtigator will generally have recourſe. "We take, ſays he, for the meaſure, (as it is the effect) of exerted mechanical power, the quantity of motion which it produces (or whoſe accumulation it prevents) by its uniform exertion during ſome given time. We ſay *uniform exertion*, not becauſe this uniformity is neceſſary, but only becauſe, if any variation of the exertion has taken place, it muſt be known in order to judge of the power."

Smeaton's meaſure of mechanical power and effect. He ſays it is as the product of the weight into the height paſſed through in either caſe;

rectified by Profeſſor Robiſon.

A ſingle

Smeaton's meaſure is not applicable to an horſe ſuſtaining (but not raiſing) a weight.

A ſingle inſtance may be adduced, to which the meaſure of Mr. Smeaton is inapplicable, and in which we muſt have recourſe to ſome ſuch meaſure as that mentioned by Profeſſor Robiſon. Suppoſe that a horſe while ſtanding ſtill ſuſtains by means of a rope and ſimple fixed pulley, a maſs of a hundred-weight, and thus keeps it ſuſpended at the top of a well, for the ſpace of a minute. Neither the animal nor the weight moves, but ſhall we therefore ſay, in conformity, as it would ſeem, with Mr. Smeaton's meaſure, that there is no power expended, and no effect produced? On the contrary we know there *is* a power expended, and that the effort if ſufficiently long continued would completely tire the horſe. The effect which is produced is the annihilation of the ſimultaneous action of gravity upon the ſuſpended maſs; conſequently, the effect produced is equal, and contrary to the momentum that would be generated by gravity in the ſpace of a minute. So that $60\times 32\frac{1}{6}\times 112=216160$, is the proper repreſentative of the power expended, as well as of the work done. Were the rope to be cut and the weight ſuffered to fall for a minute, the ſame number would likewiſe denote the labour of the horſe in reſtoring it to its original place, provided that could be accompliſhed in an equal ſpace of time, without the horſe changing his ſituation.

General ſtatement.

It may not, perhaps, be entirely uſeleſs to ſtate this matter rather more univerſally. To this end, let M repreſent any maſs or body, $g=32\frac{1}{6}$ feet, the velocity communicated to a body falling freely in the firſt ſecond of time, and t' an indefinitely ſmall portion of any time whatever t. Then will $g\,t'$ be the velocity generated in the inſtant t', and $M\,g\,t'$ the correſponding quantity of motion; this, therefore, meaſures the effort which muſt be exerted at each inſtant to ſuſtain the weight, whether that effort be applied immediately, or through the intervention of a ſingle fixed pulley. Hence it follows, that during the whole time t, the force will have conſumed a quantity of motion equal to $\int M\,g\,t' = M\,g\,t$: that is to ſay, if t denote the time at the end of which the agent is no longer able to ſuſtain the maſs M, we may regard $M\,g\,t$ as being an adequate meaſure of the force ϕ of that agent. If the agent not only prevent the maſs from falling, but actually raiſe it with a given uniform velocity V during the whole time t, then we muſt add the quantity of motion MV to the former, which gives $\phi = M V \times M g t = M (V \times g t.)$ And laſtly, if the agent poſſeſs inertia, its maſs muſt alſo be conſidered. Thus, in the caſe of a horſe whoſe maſs is H, moving along with the velocity V during the time t, and raiſing the maſs M, we ſhall have $\phi = (M \times H) V \times M g t$. And from ſimilar principles formulæ may be inveſtigated to repreſent the power of a firſt mover in more complicated caſes.

Power expended and power uſefully employed require to be diſtinguiſhed.

It will after all, be proper to diſtinguiſh carefully between the quantity of power expended, and that portion of it which is *uſefully* employed: but a due conſideration of this would too widely extend the limits of the preſent communication. Indeed I ought to apologize to yourſelf, and the ſcientific part of your readers, for dwelling ſo long as I have done upon topics which are well known to all who are converſant in the theory of mechanics: but if thoſe, for whoſe uſe this letter is chiefly intended, ſhall derive ſome preciſe information, or add to the ſtock of their practical knowledge, by any hints of mine, I ſhall not fear being heavily cenſured for having entered thus into minutiæ.

I am, Sir,

Your's very reſpectfully,

OLINTHUS GREGORY.

Royal Mil. Academy, Woolwich,
June 10th, 1805.

II.

An Account of ſome Analytical Experiments on a Mineral Production from Devonſhire conſiſting principally of Alumine and Water. By HUMPHRY DAVY, *Eſq. F. R. S. Profeſſor of Chemiſtry in the Royal Inſtitution. From the Philoſophical Tranſactions in* 1805.

I. *Preliminary Obſervations.*

Hiſtory of Foſſil.

THIS foſſil was found many years ago by Dr. Wavell, in a quarry near Barnſtaple: Mr. Hatchett, who viſited the place in 1796, deſcribed it as filling ſome of the cavities and veins in a rock of ſoft argillaceous ſchiſt. When firſt made known, it was conſidered as a zeolite; Mr. Hatchett, however, concluded, from its geological poſition, that it moſt probably did not belong to that claſs of ſtones; and Dr. Babington, from its phyſical characters, and from ſome experiments on its ſolutions in acids, made at his requeſt by Mr. Stocker, aſcertained that it was a mineral body, as yet not deſcribed, and that it contained a conſiderable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general inveſtigation of its chemical nature; and that gentleman liberally ſupplied me with ſpecimens for analyſis.

II. *Senſible Characters of the Foſſil.*

Its ſenſible characters. Radiated hemiſpherical groups; white, ſilky, hard, little tenacious.

The moſt common appearance of the foſſil is in ſmall hemiſpherical groups of cryſtals, compoſed of a number of filaments radiating from a common center, and inſerted on the ſurface of the ſhiſt; but in ſome inſtances it exiſts as a collection of irregularly diſpoſed priſms forming ſmall veins in the ſtone: as yet, I believe, no inſulated or diſtinct cryſtal has been found. Its colour is white, in a few caſes with a tinge of gray or of green, and in ſome pieces (apparently beginning to decompoſe) of yellow. Its luſtre is ſilky; ſome of the ſpecimens poſſeſs ſemi-tranſparency, but in general it is nearly opaque. Its texture is looſe, but its ſmall fragments poſſeſs great hardneſs, ſo as to ſcratch agate.

Other characters.

It produces no effect on the ſmell when breathed upon, has no taſte, does not become electrical or phoſphoreſcent by heat or friction, and does not adhere to the tongue till after it has been ſtrongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loſes its hardneſs, and becomes opaque. In conſequence of the minuteneſs of the portions which it is found, few of them exceeding the ſize of a pea, it is very difficult to aſcertain its ſpecific gravity with any preciſion; but from ſeveral trials I am diſpoſed to believe, that it does not exceed 2,70, that of water being conſidered as 1,00.

III. *Chemical Characters of the Foſſil.*

Chemical habitudes. Soluble in acids and in f. alkalis.

The perfectly white and ſemi-tranſparent ſpecimens of the foſſil are ſoluble both in the mineral acids and in fixed alkaline lixivia by heat, without ſenſibly efferveſcing and without leaving any notable reſiduum; but a ſmall part remains undiſſolved, when coloured or opaque ſpecimens are expoſed to the alkaline lixivia.

A ſmall ſemi-tranſparent piece, acted on by the higheſt heat of an excellent forge, had its cryſtalline texture deſtroyed, and was rendered opaque; but it did not enter into fuſion. After the experiment it adhered ſtrongly to the tongue, and was found to have loſt more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the foſſil. When it was acted on by a heat of from 212° to 600° Fahrenheit in a glaſs tube, it gave out an elaſtic vapour, which when condenſed appeared as a clear fluid poſſeſſing a ſlight empyreumatic ſmell, but no taſte different from that of pure water.

Forge heat gave opacity, but did not fuſe it. Loſs one-fourth.

It emitted water.

The ſolution of the foſſil in ſulphuric acid, when evaporated ſufficiently, depoſited cryſtals which appeared in thin plates, and had all the properties of ſulphate of alumine; and the ſolid matter, when rediſſolved and mixed with a little carbonate of potaſh, ſlowly depoſited octahedral cryſtals of alum. The ſolid matter precipitated from the ſolution of the white and ſemi-tranſparent foſſil in muriatic acid, was in no manner acted upon by ſolution of carbonate of ammonia, and therefore it could not contain any glucine or ittria; and its perfect ſolubility without reſiduum in alkaline lixivia ſhewed that it was alumine.

Sulphuric ſolution afforded ſulphate of alumine.

Muriatic ſolution contained alumine *only*.

When the opaque *varieties* of the foſſil were fully expoſed to the agency of alkaline lixivia, the reſiduum never amounted to more than one-twentieth part of the weight of the *whole*. In the white opaque variety, it was merely calcareous earth, for when diſſolved in muriatic acid, not in exceſs, it gave a white precipitate when mixed with ſolution of oxalate of ammonia, and did not affect ſolution of pruſſiate of potaſh and iron.

The white varieties contained ſome lime;

In the green opaque variety, calcareous earth was indicated by ſolution of oxalate of ammonia: and it contained oxide of manganeſe; for it was not precipitated by ſolution of ammonia; but was rendered turbid, and of a gray colour, by ſolution of pruſſiate of potaſh and iron.

and the green, alſo manganeſe:

The reſiduum of the alkaline ſolution of the yellow variety, when diſſolved in muriatic acid, produced a ſmall quantity of white ſolid matter when mixed with the ſolution of the oxalate

the yellow, a trace of iron.

of ammonia, and gave a light yellow precipitate by expoſure to ammonia; but after this, when neutralized, it did not affect pruſſiate of potaſh and iron, ſo that its colouring matter, as there is every reaſon to believe, was *oxide of iron.*

IV. *Analyſis of the Foſſil.*

Analyſis of 80 grains.
1. Expoſure to heat.

Eighty grains of the foſſil conſiſting of the whiteſt and moſt tranſparent parts that could be obtained, were introduced into a ſmall glaſs tube having a bulb of ſufficient capacity to receive them with great eaſe. To the end of this tube, a ſmall glaſs globe attached to another tube, communicating with a pneumatic mercurial apparatus, was joined by fuſion by means of the blow-pipe.

The bulb of the tube was expoſed to the heat of an Argand lamp; and the globe was preſerved cool by being placed in a veſſel of cold water. In conſequence of this arrangement, the fluid diſengaged by the heat, became condenſed, and no elaſtic matter could be loſt. The proceſs was continued for half an hour, when the glaſs tube was quite red.

Water came over.
19 grains
very ſlightly acid.

A very minute portion only of permanently elaſtic fluid paſſed into the pneumatic apparatus, and when examined, it proved to be common air. The quantity of clear fluid collected, when poured into another veſſel, weighed 19 grains, but when the interior of the apparatus had been carefully wiped and dried the whole loſs indicated was 21 grains. The 19 grains of fluid had a faint ſmell, ſimilar to that of burning peat; it was tranſparent, and taſted like diſtilled water: but it ſlightly reddened litmus paper. It produced no cloudineſs in ſolutions of muriate of barytes, of acetite lead, of nitrate of ſilver, or of ſulphate of iron.

2. Solution of the reſidue in ſulph. acid: precipitation and re-ſolution by alkali. One grain and a quarter of lime remained undiſſolved.

The 59 grains of ſolid matter were diſſolved in diluted ſulphuric acid, which left no reſiduum; and the ſolution was mixed with potaſh, in ſufficient quantity to cauſe the alumine at firſt precipitated again to diſſolve. What remained undiſſolved by potaſh, after being collected and properly waſhed, was heated ſtrongly and weighed; its quantity was a grain and quarter. It was white, cauſtic to the taſte, and had all the properties of lime.

3. Nitric acid was added in exceſs, and then carbonate of am-

The ſolution was mixed with nitric acid till it *became ſour.* Solution of carbonate of ammonia was then poured into it till the effect of decompoſition ceaſed. The whole thrown into a filtrating apparatus left ſolid matter, which when carefully waſhed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general reſults of the experiments, when calculated upon, indicated for 100 parts of this ſpecimen,

monia. The precip. of alumine, after ignition, was 56 grains.

Of alumine - -	70
Of lime - -	1.4
Of fluid - -	26.2
Loſs - -	2.4

Component parts.

The loſs I am inclined to attribute to ſome fluid remaining in the ſtone after the proceſs of diſtillation; for I have found, from ſeveral experiments, that a red heat is not ſufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a ſtrong white heat.

The loſs appears to have been volatilizable matter.

Fifty grains of a very tranſparent part of the foſſil, by being expoſed in a red heat for fifteen minutes, loſt 13 grains; but when they were heated to whiteneſs, the deficiency amounted to 15 grains, and the caſe was ſimilar in other trials.

Different ſpecimens of the foſſil were examined with great care, for the purpoſe of aſcertaining whether any minute portion of fixed alkali exiſted in them; but no indications of this ſubſtance could be obſerved; the proceſſes were conducted by means of ſolution of the unaltered foſſil in nitric acid; the earths and oxides were precipitated from the ſolution by being boiled with carbonate of ammonia; and after their ſeparation, the fluid was evaporated to dryneſs, and the nitrate of ammonia decompoſed by heat, when no reſiduum occurred.

The mineral contains no alkali.

A comparative analyſis of 30 grains of a very pellucid ſpecimen was made by ſolution in lixivium of potaſh. This ſpecimen loſt 8 grains by long-continued ignition, after which it eaſily diſſolved in the lixivium by heat, leaving a reſiduum of a quarter of a grain only, which was red oxide of iron. The precipitate from the ſolution of potaſh, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Other analyſes.

Several ſpecimens were diſtilled in the manner above deſcribed, and in all caſes the water collected had ſimilar properties. The only teſt by which the preſence of acid matter in it could be detected, was litmus paper; and in ſome caſes the effect upon this ſubſtance was barely perceptible.

V. *General Obſervations.*

The acid matter in the water was not nitric, mur. or ſulph.

I have made ſeveral experiments with the hope of aſcertaining the nature of the acid matter in the water; but from the impoſſibility of procuring any conſiderable quantity of the foſſil, they have been wholly unſucceſsful. It is, however, evident, from the experiments already detailed, that it is not one of the known mineral acids.

It is foreign to the ſtone.

I am diſpoſed to believe, from the minuteneſs of its proportion, and from the difference of this proportion in different caſes, that it is not eſſential to the compoſition of the ſtone; and that, as well as the oxide of manganeſe, that of iron, and the lime it is only an accidental ingredient, and on this idea the pure matter of the foſſil muſt be conſidered as a chemical combination *of about thirty parts of water and ſeventy of alumine.*

Alumine has an affinity for water.

The experiments of M. Theodore de Sauſſure on the precipitation* of alumine from its ſolutions, have demonſtrated the affinity of this body for water; but as yet I believe no aluminous ſtone, except that which I have juſt deſcribed, has been found, containing ſo large a proportion of water, as thirty parts in the hundred.

Diaſpore exam. by Vauquelin, contains 80 alumine and 16 water.

The diaſpore, which has been examined by M. Vauquelin, and which loſes ſixteen or ſeventeen parts in the hundred by ignition, and which contains nearly eighty of alumine, and only three of oxide of iron, is ſuppoſed by that excellent chemiſt to be a compound of alumine and water. Its phyſical and chemical characters differ however very much from thoſe of the new foſſil, and other reſearches are wanting to aſcertain whether the part of it volatilized by heat is of the ſame kind.

Corniſh mineral reſembling the ſubject of this paper.

I have examined a foſſil from near St. Auſtle, in Cornwall, very ſimilar to the foſſil from Barnſtaple in all *its general chemical characters;* and I have been informed, that an analyſis of it, made by the Rev. William Gregor ſome months ſince, proves that it conſiſts of ſimilar ingredients.

Propoſed names.

Dr. Babington has propoſed to call the foſſil from Devonſhire *Wavellite,* from Dr. Wavell, the gentleman who diſcovered it; but if a name founded upon its chemical compoſition be preferred, it may be denominated *Hydrargillite,* from ὕδωρ water, and ἄργιλλος clay.

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* Journal de Phyſique, Tom. LII. p. 280.

On

III.

On the Aberrations of Light paſſing through Lenſes. By Mr. EZEKIEL WALKER.

Newton's diſcovery of the aberrations of coloured rays.

THE diſcovery of the aberration of the rays of light, cauſed by their unequal refrangibility, formed a new area in the ſcience of optics. It is the foundation of all *Sir Iſaac Newton's* diſcoveries in light and colours; and alſo the foundation of moſt of the uſeful improvements in the conſtruction of optical inſtruments, that have done ſo much honour to our country. And this ſcience may ſtill derive further improvements from the ſame diſcovery, not only in the conſtruction of inſtruments, but alſo in explaining ſome curious phenomena in nature.

But before I attempt to ſhow how the uſe of this property of viſion may be extended, it ſeems neceſſary to give a ſhort account of that kind of aberration which ariſes from the unequal refrangibility of the differently coloured rays of light: the other aberration, or that which is cauſed by the ſpherical figure of the lens, is not here conſidered as being inconſiderable when compared with the former.

Aberration of light that paſſes through a lens.

Therefore let A C B, *Fig.* 2, *Plate* X. repreſent a plano-convex lens; P A and R B two pencils of white or compound rays of light, falling upon it at the points A and B in a direction parallel to its axis. Alſo let A xv and B yv be the red or leaſt refrangible rays, and A gy and B qx the violet, or moſt refrangible. The red ray from A will cut the violet ray from B at the point x, and the red ray from B will cut the violet ray from A at the point y; through theſe interſections draw the line xy, and this line will be the diameter of the leaſt circular ſpace into which all the rays that fall upon the lens, parallel to its axis, can be collected. And this circle, which for brevity's ſake is called the circle of aberration, is the true focus of the lens or place where the image of the object is formed.

Let the ſine of incidence going out of glaſs be n, the ſines of refraction (into air) of the leaſt and the moſt refrangible rays be p and q; then if a plano-convex lens be expoſed with the plane ſide to the ſun, the diameter of the circle of aberration xy (or image of the ſun formed of rays of different refrangibility) is to the diameter of the lens A B, as $q-p$ to $q \times p-2n$.*

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* This theorem is well known to mathematicians.

From this given ratio of A B to xy it follows, that the image of the ſun within a teleſcope varies with the aperture of the object glaſs.

Sir Iſaac Newton found by moſt accurate experiments that where the ſine of incidence was 50, the ſines of refraction of the red and the violet rays were 77 and 78. Hence $q-p$ is to $q+p-2n$ as 1 to 55. And therefore A B : xy : : 55 : 1; or $xy = \frac{AB}{55}$.

To elucidate this theorem by examples, let the diameter of the object glaſs of a teleſcope, which is 4 inches, be contracted to 3 inches, and afterwards to 2; then the diameters of the circles by aberration formed by parallel rays, will be $\frac{4}{55} = .072$, $\frac{3}{55} = ,054$, and $\frac{2}{55} = .036$ reſpectively.

The ſame Property of Viſion demonſtrated otherwiſe.

Thus, let n repreſent the ſine of incidence, and p and q the ſines of refraction, as before.

The ſine of incidence of every ray, is to its ſine of refraction in a given ratio.*

And the ſine of incidence of the extreme ray P A, varies with the aperture of the lens. For n becomes leſs as P A approaches the axis of the lens E v.

Therefore the ſines of refraction p and q, the angle x A y, and its ſubtenſe xy increaſe or decreaſe with A B. Conſequently the image of the ſun or moon, upon the retina increaſes or decreaſes in magnitude with the pupil of the eye.

Deduction;

Now as the rays of artificial light are differently refrangible, it is evident from the given ratio of A B to xy, in which they increaſe or decreaſe at the ſame time that the image of a candle formed in the focus of a convex lens decreaſes with the aperture of the glaſs. For the rays of the ſun and the light of a candle are both governed by the ſame law, in the formation of images in the focus of a lens; but this law does not obtain in the ſame degree in both objects, in conſequence of the rays of the latter being in a more diverging ſtate than thoſe of the former.

—applied in ſupport of the author's experiments.

Hence the truth of the reſult of my experiments, which were publiſhed in Vol. IX. page 164 of this Journal, is proved from the diſcoveries of *Sir Iſaac Newton.*

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* Newton's Optics, page 64.

What I have further to advance on this ſubject muſt be reſerved until ſome future opportunity, as it would exceed the limits of this paper.

E. WALKER.

Lynn, May 16, 1805.

IV.

New Method of decompoſing the Sulphate of Barytes for preparing the Muriate of that Earth, and Preparation of the Muriate. By Mr. GOETTLING.*

THE muriate of barytes is now in ſuch general uſe, that every improvement in the mode of preparing it muſt meet a favourable reception. This will render the new method of Mr. Goettling acceptable to the public.

Muriate of barytes much uſed.

The decompoſition of ſulphate of barytes by means of charcoal requires a ſtrong fire continued a long time, and never ſucceeds completely. This is owing on the one hand to the ſtrongly oxigenated quality of the acidifying principle in the ſulphuric acid, ſo that in its tranſlation to the charcoal it gives out but little caloric; and on the other hand to the difficulty of imparting a certain degree of heat to a mixture, into which a large quantity of a body that is ſo bad a conductor of heat as charcoal enters. To remedy the firſt of theſe defects, I had already propoſed to increaſe the proportion of charcoal a little, and to incorporate with the mixture of charcoal and ſulphate of barytes a twentieth of nitrate of potaſh. To remedy the ſecond, Mr. Goettling adviſes to add muriate of ſoda to the mixture, which ſerves at the ſame time as a conductor of heat and a flux. The following is his method.

Decompoſition of ſulphate of barytes by charcoal troubleſome and incomplete.

Remedies: to uſe more charcoal and nitrate of potaſh;

—and muriate of ſoda.

Four parts of native ſulphate of barytes in fine powder are to be mixed with one part of muriate of ſoda and half a part of charcoal powder. This mixture is to be preſſed hard into a Heſſian crucible, and expoſed for an hour and half to a red heat in a good wind furnace. After it has grown cold, the

Mr. Goettling's proceſs. Sulphate of barytes with muriate of ſoda and ſome charcoal are heated together.

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* Tranſlated from Van Mons's *Journal de Chimie*, Vol. VI. p. 80. Originally publiſhed in the *Taſchen-Buch fuer Scheidekuenſtler*.

The ſaline maſs is then to be diſſolved.

maſs is to be reduced to a coarſe powder, and boiled for a moment with ſixteen parts of water. The liquor is then to be filtered, and kept in well ſtopped bottles.

The time of expoſure to heat may be ſhortened to one half, if the quantity of muriate of ſoda be doubled, and the matter occaſionally ſtirred. In this caſe too, double the quantity of water ſhould be uſed to lixiviate the maſs.

An addition of muriatic acid expels the ſulphureous acid and leaves muriates of barytes and of ſoda.

To prepare muriate of barytes with this lixivium of ſulphuret of barytes, which at the ſame time holds in ſolution muriate of ſoda, muriatic acid is to be added in ſeparate portions till ſulphurated hidrogen gas is no longer extricated. The liquor is then to be filtered, a little hot water is to be poured on the reſiduum, and the liquor is to be evaporated to a pellicle. The lixivium being then filtered afreſh, is to be ſet to cryſtallize; the muriate of ſoda, which is much more ſoluble in water than the muriate of barytes, and not more ſoluble with heat than without, is not depoſited by cooling, and the muriate of barytes cryſtallizes alone.

Theſe cryſtals of muriate of barytes are ſeparated by cooling after evaporation.

The remaining lixivium is to be evaporated and ſet to cryſtallize again, and this is to be repeated till no more cryſtals of muriate of barytes are formed.

The barytic ſalt is perfectly white and pure.

The barytic ſalt thus obtained, if care be taken not to employ an exceſs of muriatic acid, is perfectly white, on account of the hydroſulphuret, by which the iron and other metallic ſubſtances are precipitated. To be more certain that it contains no muriate of ſoda, the different products of the cryſtallization ſhould be mixed together, diſſolved, and re-cryſtallized.

V.

On the Action of Platina and Mercury upon each other. By RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A, &c. From the Philoſophical Tranſactions for* 1805, *p.* 104.

Freyberg, June 3d, 1804.

Reference to the author's paper on palladium.

ON the 12th of May, 1803, I had the honour of preſenting a Paper to the Royal Society,* the object of which was to diſcover the nature of palladium, a ſubſtance juſt then announced not ſenſibly acted upon by the ſame reagent. The inſolubility of muriate of ſilver might be alledged as the cauſe of this, if I had not tried the experiment by pouring nitrate of ſilver into green muriate of iron, in which caſe all the ſubſtances were preſented to each other in ſolution. The reſult was not reduction, but muriate of ſilver and nitrate of iron. This fact reſts upon a much more extenſive baſis than mere mechanical circumſtances; and, if purſued with the intention it deſerves, it would lead us into the wide expanſe of complicated affinities and their relations. From reaſoning alone we ſhould be diſpoſed to think that an acid, ſo eaſily decompoſed as the nitric, would be ſufficient to prevent the reduction of a metal which it can diſſolve. But on the one hand it can ſpend its oxygen upon a part of the oxide of the green ſulphate of iron, while on the other its affinity for oxide of ſilver is not powerful enough to retain it, when there is another part of the oxide of iron preſent to deprive it of oxygen. But the affinity of muriatic acid for oxide of ſilver, one of the ſtrongeſt at preſent known, is ſufficient to counterbalance all the other forces. There are many other inſtances of the ſame kind.

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* Inſerted in our Journal, Vol. VII. 85, 176.

Mercury in nitric acid is precipitated (metallic) by gr. ſulph. of iron.

If then a ſolution of green ſulphate of iron be brought into contact with either ſoluble or inſoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be diſſolved in nitric acid, and green ſulphate of iron be added, the mercury is precipitated in the metallic ſtate.

Theſe remarkable facts modify the reſults formerly ſtated.

Theſe experiments are much ſtronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general conſideration but in their application to the preſent ſubject. They moſt materially modify and are indiſpenſable to the accuracy of the reſults I formerly ſtated; but I was not aware of them at the time I firſt engaged in the inveſtigation of this ſubject. I can alſo now explain a very material difference between ſome proportions obſerved by M. Richter and myſelf in an experiment which that chemiſt had made as a repetition of one of mine.

Precipitation of gold and mercury by gr. ſulphate of iron: repeated by Richter.

I had poured a ſolution of green ſulphate of iron into a ſolution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate conſiſting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment;

ment; that is, he uſed 100 of gold and 500 of mercury, and obtained a precipitate weighing 102. He is ſurpriſed at the difference of weight between our reſults, which might be owing to his *method of repeating* the experiment; but the real cauſe of this difference lies, as I ſuppoſe, in my having accidentally uſed nitrate inſtead of muriate of mercury. I had never obſerved that with mercury and ſilver this operation had failed, and it muſt have been, becauſe, on account of the known effect of muriatic ſalts upon thoſe of ſilver, I had naturally avoided uſing a muriate of mercury.

The ſtate of oxidation of the nitrate of mercury uſed with the ſolution of gold is of conſequence: If the minimum of oxidation prevails, calomel and metallic gold fall down;

But the ſtate of the nitrate of mercury which is uſed with a ſolution of gold is not indifferent. As green ſulphate of iron reduces mercury when diſſolved in nitric acid, as well as gold, it is neceſſary to mix the ſolutions of thoſe metals before the green ſulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the ſolutions of gold and mercury. Calomel is produced by the muriatic acid of the ſolution of gold and the oxide of mercury; whilſt the gold is reduced to the metallic ſtate by a portion of the oxide of mercury becoming more oxidized, and forming the ſoluble muriate. The precipitate conſiſts of calomel, of metallic gold, and of a very ſmall portion of mercury which I believe to be in the ſame ſtate; my reaſon for thinking ſo, is, that I have often obſerved, that a glaſs veſſel in which I had ſublimed ſome of it, was lined with a thin gray metallic coat.

but if the maximum, then nothing falls till the green ſulphate of iron is added.

If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green ſulphate of iron is added. But at any rate the precipitation of gold and mercury, or of ſilver and mercury by green ſulphate of iron, cannot be adduced as an argument to ſupport the affinity of theſe metals, ſince the effect is the ſame, whether they are ſeparate or united.

Theſe preliminary conſiderations were neceſſary as well for the rectification of my former experiments as for the purſuit of my preſent object; and now to return to platina.

Experiments with platina.

1. Much of the highly oxid. ſol. of mercury poured into a mixed ſolution of plat.

Exper. 1. If a ſolution of highly oxidized nitrate of mercury be poured into a mixed ſolution of platina and green ſulphate of iron, the firſt action which takes place paſſes between the muriatic acid of the ſolution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in ſolution. This effect makes it advantageous to uſe a greater quantity of the ſolution of mercury than is merely capable of drawing down the given quantity of platina along with itſelf in the form of a metallic precipitate. When this precipitate is waſhed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but thoſe of the mercury and the green ſulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer diſſolves any part of it, there ſtill remains more undiſſolved matter than the original weight of the platina uſed in the experiment. By expoſure to heat little more is left in general than the original platina; and ſometimes even a diminution may be obſerved; for as the experiment is not attended with uniform ſucceſs, it does not always happen that the whole of the platina is precipitated, but a portion of it will ſometimes reſiſt the action of the green ſulphate of iron, even when ſufficient mercury has been uſed. Before the precipitate has been expoſed to heat it is diſſolved more eaſily than platina by nitro-muriatic acid; and the ſolution when nearly in a neutral ſtate gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a ſolution of green ſulphate of iron.

and gr. ſulph. of iron, throws down platina and mercury united.

Part of the latter is defended from nitric acid.

Heat drives off the mercury.

The compound precipitate is more ſoluble than plat. in n. m. acid, and gives much metal. precip. by add. of ſ. of gr. ſ. of iron.

Exper. 2. When a mixed ſolution of platina and mercury is precipitated by metallic iron, a quantity equal to the ſum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate ſo formed, the original weight of platina, together with a conſiderable increaſe, remains behind, nor can nitric acid ſenſibly diminiſh it. It yields more eaſily than platina to the action of nitro-muriatic acid, and its ſolution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green ſulphate of iron. If this precipitate be expoſed to a ſtrong heat after it has been boiled with nitric acid, it loſes a great part of its weight, and the platina alone will generally be found to remain.

Exp. 2. Metallic iron throws down the pl. and merc. from a mixed ſolution. Nitric acid does not deprive this metal of all its mercury. It is more ſoluble in n. m. acid; and precip. by gr. ſ. of iron. Heat uſually expels the mercury from this.

Exper. 3. When a quantity of ammoniacal muriate of platina is treated according to the method of Count Muſſin Puſhkin to form an amalgam, and, after being rubbed for a conſiderable time with mercury, is expoſed in a crucible to a heat gradually increaſed till it becomes violent, a metallic powder remains in the

Exp. 3. Amalgam (from ammon. mur. of plat.) ſtrongly heated, leaves a powder, acted on by n. m. acid, and copiouſly precip. by gr. ſ. of iron.

the crucible. This powder is acted upon by nitro-muriatic acid, and when the ſolution is neutralized, a copious precipitate is formed upon the addition of green ſulphate of iron. This effect takes place even after the metal has been fuſed in the manner deſcribed in the former part of this paper.

Exp. 4. Sulphur added in the laſt Exp. cauſes a greater precip.

Exper. 4. If ſulphur be added to the ingredients recommended by Count Muſſin Puſhkin, and the whole treated as in the laſt experiment, the quantity of precipitate cauſed by green ſulphate of iron in the nitro-muriatic ſolution of the button which reſults from the operation, is generally more conſiderable.

Exp. 5. Sulphur rubbed with am. m. of plat. eaſily melts. Mercury being added, ſtrong fuſion gives a button, ſol. in n. m. acid and precipitable by gr. ſ. of iron.

Exper. 5. If ſulphur be rubbed for ſome time with ammoniacal muriate of platina, and the mixture be introduced into a ſmall Florence flaſk, it can be melted on a ſand-bath. If mercury be then thrown into it, and the whole be well ſtirred together and heated, it may afterwards be expoſed to a very ſtrong fire and melted into a button. If this be diſſolved in nitro-muriatic acid, it will give a precipitate, as in the former caſes, by green ſulphate of iron.

Exp. 6. The precip. from ſol. of plat. and merc. by ſulph. hydrogen being fuſed affords a ſol. precip. by gr. ſ. of iron.

Exper. 6. If a current of ſulphuretted hydrogen gas be ſent through a mixed ſolution of platina and mercury, and the precipitate which enſues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any ſulphur. Green ſulphate of iron cauſes a precipitate in the ſolution of this metal alſo.

Exp. 7. So likewiſe the precip. by phoſphate of ammon.

Exper. 7. If to a mixed ſolution of platina and mercury, phoſphate of ammonia be added, a precipitate takes place. If this be collected and reduced, it will be acted upon by green ſulphate of iron poured into its ſolution, in the ſame manner as the metallic buttons in the preceding examples.

Exp. 8. Nitr. of merc. at mini of oxidiz. precipitates mur. of pl. The metallic compound diſſolved in n. m. acid is precipitable by gr. ſ. of iron.

Exper. 8. I have already mentioned that when a ſolution of nitrate of mercury, at the minimum of oxidizement, is poured into a ſolution of muriate of platina, a mercurial muriate of platina is precipitated. The ſupernatant liquor may be decanted and the reſiduum waſhed; if this be reduced and afterwards diſſolved in nitro-muriatic acid, it will yield a precipitate with green ſulphate of iron. This method appears to me to be the neateſt for combining platina and mercury, as the action which takes place is independent of every ſubſtance except the metals themſelves.

Exper. 9. One of the moſt delicate teſts that I have obſerved in chemiſtry is recent muriate of tin, which detects the preſence of the ſmalleſt portion of mercury. When a ſingle drop of a ſaturate ſolution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a ſaturate ſolution of recent muriate of tin are added, the liquor becomes a little turbid, and of a ſmoke-gray colour. If theſe 500 grains of liquid be diluted with ten times their weight of water, the effect is of courſe diminiſhed, but ſtill it is perceptible. I had on a former occaſion obſerved the action of recent muriate of tin upon a ſolution of platina. If a ſolution of recent muriate of tin be poured into a mixed ſolution of platina and mercury, not too concentrated, it can hardly be diſtinguiſhed from a ſimple ſolution of platina. But if too much mercury be preſent, the exceſs is acted upon as mercury; and the liquor aſſumes a darker colour than with platina alone.

Exp. 9. Recent muriate of tin is the moſt delicate teſt of mercury. It does not indicate the mercury in a mixed ſolution of that metal and platina.

From all theſe experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic ſtate by green ſulphate of iron. By *Experiments* 1 and 2, it is proved, 1ſt, That platina can protect a conſiderable quantity of mercury from the action of nitric acid; and 2dly, That mercury can increaſe the action of nitro-muriatic acid upon platina. From *Experiments* 3, 4, 5, 6, 7, 8, it appears that mercury can combine with platina in ſuch a manner as not to be ſeparated by the degree of heat neceſſary to fuſe the compound, ſince after the fuſion it retains that property, which is eſſentially characteriſtic of the preſence of mercury in a ſolution of platina. The 8th *Experiment* proves that the action of mercury upon platina is not confined to the metallic ſtate; but that theſe metals can combine and form an inſoluble triple ſalt with an acid which produces a very ſoluble compound with platina alone. The 9th *Experiment* ſhows that platina can retain in ſolution a certain quantity of mercury, and prevent its reduction by a ſubſtance which acts moſt powerfully to that effect, when platina is not preſent. That part of the general poſition therefore which is the object of this paper is proved, if theſe experiments, upon being repeated by other chemiſts, ſhall be found to be accurate.

Hence, 1, 2. Platina protects mercury from nitric acid, and mercury renders platina more ſubject to nitro-mur. acid: 3—8. Platina retains mercury in the ſtrong heat of fuſion. 8. Mercury and platina act on each other in ſaline compoſition. 9. Platina defends mercury in ſolution from being reduced by m. of tin.

One or two of the above experiments ſeem to be in contradiction to ſome that I have ſtated in my paper upon palladium;

Remark on palladium.

for in the preſent examples platina protects mercury againſt the action of nitric acid; whereas in palladium the mercury is not only acted upon itſelf, but it conduces to the ſolution of platina in the ſame acid. I am well aware of this objection; but confining myſelf to my preſent object, I ſhall wave all further diſcuſſion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemiſtry, that the ſtrongeſt affinities are thoſe, which produce in any ſubſtance the greateſt deviation from its uſual properties.

The compounds of platina and mercury are ſubject to great variations, &c.

When a button of the alloy of platina and mercury as prepared by any of the above methods, is diſſolved in nitromuriatic acid, and afterwards precipitated by green ſulphate of iron, the entire quantity of the alloy uſed is ſeldom obtained. A conſiderable portion of platina reſiſts the action of green ſulphate of iron, and remains in ſolution. This may be looked upon as the exceſs of platina, and can be recovered by a plate of iron. Hence it appears that leſs mercury is fixed, than can determine the precipitation of the entire quantity of platina; yet in this ſtate it can draw down a greater quantity of the latter, than when it is merely poured into a mixed ſolution of platina, not before ſo treated. Indeed the whole of theſe experiments tend, not only to ſhow that theſe two metals exerciſe a very powerful action upon each other, but that they are capable of great variation in the ſtate of their combination; and alſo that ſubſtances poſſeſſing different properties have reſulted from my attempts to combine platina with mercury.

Inveſtigation of the quantity of mercury thus fixed. It is ſuppoſed or inferred to be about 17 mercury to 83 platina, with the ſpecific gravity 16.

This obſervation furniſhed me with a method of aſcertaining, or at leaſt of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been ſeldom attended with full ſucceſs, I mention the reſult with the entire conſciouſneſs of the uncertainty to which it is ſubject. I obſerved the increaſe of weight, which the original quantity of platina had acquired in ſome caſes after it had been treated with mercury, and fuſed into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green ſulphate of iron from a ſolution of this alloy, and ſuppoſed it to contain the whole quantity of mercury found as above. But, even if attended with complete ſucceſs, there is a chemical reaſon which muſt make us refuſe our aſſent to this eſtimate. It is poſſible, and not unlikely, that a portion of mercury may be retained in ſolution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean reſult, however, was that the precipitate by green ſulphate of iron conſiſted of about 17 of mercury, and 83 of platina, when the ſpecific gravity was about 16.

The author continues to think that palladium is a compound,

With regard to palladium, leſt it ſhould be ſuppoſed that either my own obſervations, or thoſe of others have given me cauſe to alter my opinion; I will add, that I have as yet ſeen no arguments of ſufficient weight to convince me, in oppoſition to experiment, that palladium is a ſimple ſubſtance. Repeated failure in the attempt to form it I am too well accuſtomed to, not to believe that it may happen in well conducted operations; but four ſucceſsful trials, which were not performed in ſecret, are in my mind a ſufficient anſwer to that objection. By determining the preſent queſtion we may overcome the prepoſſeſſion conceived by many againſt the poſſibility of rendering mercury as fixed, at an elevated temperature, as other metals: we may be led to ſee no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the viſions of alchemy on the one hand, and the equally unphiloſophical prejudices on the other, which they are likely to create. In the courſe of experiments juſt now related, I have ſeen nothing but what tends to confirm my former reſults, yet the only means which I can, after all, preſcribe for ſucceeding, is perſeverance.

ſince four ſucceſsful experiments were made of forming it.

Platina, purified in Fourcroy and Vauquelin's method, is equally combinable with mercury.

To aſcertain whether the opinion of Meſſ. Fourcroy and Vauquelin, that the new metal was the principal ingredient in palladium had any juſt foundation, I obſerved the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

Theſe chemiſts and M. Richter have promiſed to purſue the ſubject.

I might have added ſome more experiments to corroborate the evidence I have adduced to prove my aſſertion of the fixation of mercury by platina; but Meſſ. Vauquelin and Fourcroy have promiſed the Inſtitute of France a continuation of their reſearches, and M. Richter concludes his paper with ſaying that he will return to the ſubject. From the labours of ſuch perſons ſome great and important fact muſt iſſue, and I hope that the preſent ſubject will not be excluded from their conſideration. The facts contained in this paper cannot be ſubmitted to too ſevere a ſcrutiny; and no judge can be more rigid or more competent than the very perſon who was the firſt to doubt my former experiments. But it is neceſſary to be obſerved by whoever ſhall think them worth the trouble of verifying, that even theſe experiments are liable to fail unleſs proper precautions are uſed: that I have never operated upon leſs than one hundred grains; and that the reſults, which I have ſtated, however ſimple they may appear, have been the conſtant labour of ſome weeks.

POSTSCRIPT.

Dr. Wollaſton's palladium in crude platina proves nothing; and it may have been the product of the amalgamating proceſs

Since this paper was written Dr. Wollaſton has publiſhed ſome experiments upon platina. He has found that palladium is contained in very ſmall quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. Wollaſton. I have not yet ſeen a copy of his paper; but I ſhall merely obſerve here that, whatever be the quantity of palladium found in a natural ſtate, no concluſion can be drawn as to its being ſimple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events, the amalgamation to which platina is ſubmitted before it reaches Europe, is ſufficient to account for a ſmall portion of palladium.

VI.

Method of obviating the Neceſſity of Lifting Ships. By Mr. ROBERT SEPPINGS, *of Chatham Yard**.

Great ſaving and advantage of ſuſpending inſtead of lifting ſhips.

THE method here to be deſcribed of ſuſpending, inſtead of lifting, ſhips, for the purpoſe of clearing them from their blocks; affords a very great ſaving to the public; and abridges two-thirds of the time formerly uſed in this operation. From the ſaving of time another very important advantage is derived, namely, that of enabling large ſhips to be docked, ſuſpended, and undocked, the ſame ſpring tides. Without enumerating the inconveniencies ariſing, and, perhaps, injuries, which ſhips are liable to ſuſtain, from the former practice of lifting them, and which are removed by the preſent plan; that which relates to manual labour deſerves particular attention; twenty men being ſufficient to ſuſpend a firſt rate, whereas it would require upwards of 500 to lift her. The ſituation which Mr. Seppings held in Plymouth-yard, attached to him, in a great degree, the ſhoring and lifting of ſhips, as well as the other practical part of the profeſſion of a ſhipwright. Here he had an opportunity of obſerving, and indeed it was a ſubject of general regret, how much time, expenſe, and labour, were required in lifting a ſhip, particularly ſhips of the line. This induced him to conſider whether ſome contrivance could not be adopted to obviate theſe evils. And it occurred to him, that if he could ſo conſtruct the blocks on which the ſhip reſts, that the weight of the ſhip might be applied to aſſiſt in the operation, he ſhould accompliſh this very deſireable end. In September 1800, the ſhoring and lifting the San Joſef, a large Spaniſh firſt-rate. then in dock at Plymouth, was committed to his directions; to perform which, the aſſiſtance of the principal part of the artificers of the yard was requiſite. In conducting this buſineſs, the plan, which will be hereafter deſcribed, occured to his mind; and from that time, he, by various experiments, proved his theory to be correct: the blocks, conſtructed by him, upon which the ſhip reſts, being ſo contrived, that the facility in removing them, is proportionate to the quantity of preſſure; and this circumſtance is always abſolutely under command, by increaſing or diminiſhing the angle of three wedges, which conſtitute one of the blocks; two of which are horizontal, and one vertical. By enlarging the angle of the horizontal wedges, the vertical wedge becomes of conſequence more acute; and its power may be ſo increaſed, that it ſhall have a great tendency to diſplace the horizontal wedges, as was proved by a model, which accompanied the ſtatement to the Society; where the power of the ſcrew is uſed as a ſubſtitute for the preſſure of the ſhip.

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* From the Tranſactions of the Society of Arts, who voted him the gold medal, 1804.

Twenty men ſupply the place of 500.

Hiſtory.

The contrivance is a block, compounded of two horizontal wedges, acting under an obtuſe vertical wedge, which by the ſhip's weight, can, if needful, diſplace the others.

Mr. Seppings cauſed three blocks to be made of hard wood agreeable to his invention, and the wedges of various angles. The horizontal wedges of the firſt block were nine degrees; of the ſecond, ſeven; and of the third, five; of courſe, the angle of the vertical wedge of the firſt block was 162 degrees; of the ſecond 166; and of the third, 170 Theſe blocks, or wedges,

Experiments.

Angles of the wedges.

wedges, were well executed, and rubbed over with ſoft ſoap for the purpoſe of experiment. They were then placed in a dock, in his Majeſty's yard at Plymouth, in which a ſloop of war was to be docked; on examining them after the veſſel was in, and the water gone, they were all found to have kept their ſituations, as placed before the ſhip reſted upon them. Shores in their wake were then erected to ſuſtain the ſhip, prior to the ſaid blocks being taken from under the keel. The proceſs of clearing them was, by applying the power of battering-rams to the ſides of the outer ends of the horizontal wedges; alternate blows being given fore and aft; by which means they immediately receded, and the vertical wedges were diſengaged. It was obſerved, even in this ſmall ſhip, that the block which was formed of horizontal wedges of nine degrees, came away much eaſier than thoſe of ſeven, and the one of ſeven, than that of five. In removing the aforeſaid blocks by the power of the battering-rams, which were ſuſpended in the hands of the men employed, by their holding ropes paſſed through holes for that purpoſe, it was remarked by Mr. Seppings, that the operation was very laborious to the people; they having to ſupport the weight of the battering-rams, as well as to ſet them in motion. He then conceived an idea of affixing wheels near the extremity of that part of the rams, which ſtrikes the wedges. This was done before the blocks were again placed; and it has ſince been found fully to anſwer the purpoſe intended, particularly in returning the horizontal wedges to their original ſituations, when the work is performed for which they were diſplaced; the wheels alſo giving a great increaſe power to the rams, and decreaſe of labour to the artificers; beſides which, the blows are given with much more exactneſs. The ſame blocks were again laid in another dock, in which a two-decked ſhip of the line was docked. On examination they were found to be very ſeverely preſſed, but were removed with great eaſe. They were again placed in another dock, in which a three-decked ſhip of the line was docked. This ſhip having in her foremaſt and bowſprit, the blocks were put quite forward, that being the part which preſſes them with the greateſt force. As ſoon as the water was out of the dock, it was obſerved, that the horizontal wedges of nine and ſeven degrees had receded ſome feet from their original ſituations. This afforded Mr. Seppings a ſatisfactory proof, which experience has ſince demonſtrated, (though

A ſloop reſted upon them, and ſhored up.

The horizontal wedges were then driven out,

by battering-rams,

ſupported on wheels.

Other experiments.

It was found that the weight of the veſſel was capable of preſſing out the wedges;—

(though many perſons before would not admit of, and others could not underſtand, the principle) that the facility of removing the blocks or wedges, was proportionate to the quantity of preſſure upon them. The block of five degrees kept its place, but was immediately cleared, by applying the power of the battering-rams to the ſides of the outer ends of the horizontal wedges. The above experiments being communicated to the Navy Board, Mr. Seppings was directed to attend them, and explain the principle of his invention; which explanation, farther corroborated by the teſtimonials of his then ſuperior officers, was ſo ſatisfactory, that a dock was ordered to be fitted at Plymouth under his immediate directions. The horizontal wedges in this, and in the other docks, that were afterwards fitted by him, are of caſt iron, with an angle of about five degrees and a half, which, from repeated trials, are found equal to any preſſure, having in no inſtance receded, and, when required, were eaſily removed. The vertical wedge is of wood, lined with a plate of wrought iron, half an inch thick. On the bottom of the dock, in the wake of each block, is a plate of iron three quarters of an inch thick, ſo that iron at all times acts in contact with iron.

and therefore that if made of the proper angle they would be ſecure, and always capable of being driven out.

The beſt angle is about 5½ degrees for caſt iron wedges.

The placing the ſuſtaining ſhores, the form and ſizes of the wedges, and battering-rams, &c. alſo the proceſs of taking away, and again re-placing, the wedges of which the block is compoſed, are alſo exemplified by a model.

The dock being prepared at Plymouth, in Auguſt, 1801, the Canopus, a large French 80-gun ſhip, was taken in, and reſted upon the blocks; and the complete ſucceſs of the experiment was ſuch, that other docks were ordered to be fitted at Sheerneſs and Portſmouth dock-yards, under Mr. Seppings's directions. At the former place a frigate, and at the latter a three-decked ſhip, were ſuſpended in like manner. This happened in December, 1802, and January, 1803; and the reports were ſo favourable, as to cauſe directions to be given for the general adoption of theſe blocks in his Majeſty's yards. This invention being thought of national conſequence, with reſpect to ſhips, but particularly thoſe of the navy, government has been pleaſed to notice and reward Mr. Seppings for it.

A large 80 gun ſhip ſuſpended by this method.

The time required to diſengage each block, is from one to three minutes after the ſhores are placed: and a firſt-rate ſits

Each block may be diſengaged in three minutes.

on

It is not required to ſuſpend the ſhip in all caſes. For the repairs may be done by ſucceſſive removal of blocks.

The lifting of ſhips was a frequent operation in the navy.

on about fifty blocks. Various are the cauſes for which a ſhip may be required to be cleared from her blocks, viz. to ſhift the main keel; to add additional falſe keel; to repair defects; to caulk the garboard ſeams, ſcarples of the keel, &c. Imperfections in the falſe-keel, which are ſo very injurious to the cables, can in the largeſt ſhip be remedied in a few hours by this invention, without adding an additional ſhore, by taking away blocks forward, amid-ſhips, and abaft, at the ſame time; and when the keel is repaired in the wake of thoſe blocks, by returning them into their places, and then by taking out the next, and ſo on in ſucceſſion. The blocks can be replaced in their original ſituations, by the application of the wheel battering-rams to the wedges, the power of which is ſo very great, that the weight of the ſhip can be taken from the ſhores that were placed to ſuſtain her. There were one hundred and ſix ſhips of different claſſes, lifted at Plymouth dock-yard, from the 1ſt of January, 1798, to the 31ſt of December, 1800; and, had the operation of lifting taken leſs time, the number would have been very conſiderably increaſed; for the ſaving of a day is very frequently the cauſe of ſaving the ſpring tide, which makes the difference of a fortnight. The importance of this expedition, in time of war, cannot be ſufficiently eſtimated.

This invention is of value in other undertakings.

Fid of a top gallant maſt,

This invention may be applied with great advantage, whenever it is neceſſary to erect ſhores, to ſupport any great weights, as, for inſtance, to prop up a building during the repair of its foundation, &c. Captain Wells, of his Majeſty's ſhip Glory, of 98 guns, uſed wedges of Mr. Seppings's invention for a fid of a top-gallant maſt of that ſhip. In 1803, the top-gallant maſts of the Defence, of 74 guns, were fitted on this principle by Mr. Seppings: and, from repeated trials, ſince ſhe has been cruizing in the North Sea, the wedge fids have been found in every reſpect to anſwer.

applied by Capt. Wells.

But it is Mr. Seppings's wiſh that it ſhould be underſtood, that the idea of applying this invention to the fid of a top-gallant maſt originated with Capt. Wells, who well underſtood the principle, and had received from him a model of the invention.

Manœuvre of ſtriking the maſt, &c.

When it is required to ſtrike a top-gallant maſt, the top ropes are hove tight, and the pin which keeps the horizontal wedges in their place, is taken out, by one man going aloft for that purpoſe; the other horizontal wedge is worked in the fid, as ſhown in the drawing and model that accompany this ſtatement. The upper part of the fid hole is cut to form the vertical wedge. The advantage derived from fidding top-gallant maſts in this way is, that they can be ſtruck at the ſhorteſt notice, and without ſlacking the rigging, which is frequently the cauſe of ſpringing and carrying them away, particularly thoſe with long pole heads. The angle of the horizontal wedges for the fids of maſts ſhould be about twenty degrees.

It can now be done without ſlacking the rigging.

Certificates.

The above Account was accompanied with Certificates from Sir John Henſlow, Surveyor of the Navy; Mr. M. Didram, maſter-ſhipwright of Portſmouth-Yard; and Mr. John Carpenter, foreman of Sheerneſs Dock-yard, confirming Mr. Seppings's ſtatement.

Reference to the Engraving of Mr. Seppings's method of obviating the neceſſity of lifting Ships. Plate XI.

Deſcription and reference to the plate, &c.

This plan and ſection of a ſeventy-four gun ſhip deſcribes the method of obviating the neceſſity of lifting ſhips, when there may be occaſion to put additional falſe keels to them, or to make good the imperfections of thoſe already on; alſo when it may be neceſſary to caulk the garboard ſeams, ſcarples the keel, &c. by which means a very conſiderable part of the expenſe will be ſaved, and much time gained. The blocks are cleared, and again returned by the following proceſs. A ſufficient number of ſhores are placed under the ſhip to ſuſtain her weight, and ſet taught, ſtationed as near the keel as the working of the battering-rams fore and aft will admit. Avoid placing any oppoſite the blocks, as they would in that caſe hinder the return of the wedges with the battering-rams. A blow muſt then be given forward on the outer end of the iron wedges with the battering-rams in a fore and aft direction, which will cauſe them to ſlide aft, as ſhown in the plan. The battering rams abaft then return the blow, and the wedges again come forward; by the repetition of this operation, the wedges will be with great eaſe cleared, and the angular block on the top will drop down. When the work is performed, the block muſt be replaced under the keel, and the wedges driven back by working the rams athwart-ſhips, as deſcribed in the ſection.

Inſtructions.

N. B. In

N. B. In returning the iron wedges, to avoid ſtraining the angular blocks, it is propoſed to leave a few of them out forward and aft, and ſtop the ſhip up, by laying one iron wedge on the other, as ſhown at Fig. 1, Plate XI.

To facilitate the buſineſs, blocks may be cleared forward and aft at the ſame time, ſufficient to get in place one length of falſe keel. If the falſe keel ſhould want repairing, it may be done without any additional ſhores, by clearing one block at a time, and when the keel is repaired in the wake of that block. return the wedges, as above directed, and clear the next, &c.

Section and Plan, Plate XI. *Fig.* 2.

Parts of the ſection and plan

A. Keelſon.
B. Ceiling.
C. Floor timber.
D. Dead or riſing wood.
E. Plank of the bottom.
F. Keel and falſe keel.
G. Angular blocks with a half-inch iron-plate bolted to them.
H. Caſt-iron wedges.
I. Iron plate of three-fourths of an inch thick on the bottom of the dock.
K. Battering-rams, with wheels, and ropes for the hands.
L. Caſt-iron wedges, having received a blow from forward.
M. Shores under the ſhip to ſuſtain her weight.

Fig. 3, repreſents part of a top-gallant maſt fitted with a wedge fid.
a. Top-gallant maſt.
b. Fid, with one horizontal wedge worked on it.
c. Moveable wedge, with the iron ſtrap and pin over it, to keep it in its ſituation.
d. Truſſel trees.

VII.

On muſcular Motion. By ANTHONY CARLISLE, *Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, November 8, 1801.*

Introduction.

ANIMAL phyſiology has derived ſeveral illuſtrations and additions, from the inſtitution of this lecture on muſcular motion; and the details of anatomical knowledge have been conſiderably augmented by deſcriptions of muſcular parts before unknown.

Still, however, many of the phenomena of muſcles remain unexplained, nor is it to be expected that any ſudden inſulated diſcovery ſhall ſolve ſuch a variety of complicated appearances.

Muſcular motion.

Muſcular motion is the firſt ſenſible operation of animal life: the various combinations of it ſuſtain and carry on the multiplied functions of the largeſt animals: the temporary ceſſation of this motive faculty is the ſuſpenſion of the living powers, its total quieſcence is death.

By the continuance of patient, well directed reſearches, it is reaſonable to expect much important evidence on this ſubject and, from the improved ſtate of collateral branches of knowledge, together with the addition of new ſources, and methods of inveſtigation, it may not be unreaſonable to hope for an ultimate ſolution of theſe phenomena, no leſs complete, and conſiſtent, than that of any other deſideratum in phyſical ſcience.

The preſent attempt to forward ſuch deſigns is limited to circumſtances which are connected with muſcular motion, conſidered as cauſes, or rather as a ſeries of events, all of which contribute, more or leſs, as conveniencies, or eſſential requiſites, to the phenomena; the details of muſcular applications being diſtinct from the objects of this lecture.

Neither the changes in muſcles during action, nor their connections with the other parts of the ſyſtem have been explained.

No ſatiſfactory explanation has yet been given of the ſtate or changes which obtain in muſcles during their contractions or relaxations, neither are their correſponding connections with the vaſcular, reſpiratory, and nervous ſyſtems, ſufficiently traced. Theſe ſubjects are therefore open for the preſent enquiry, and, although I may totally fail in this attempt to elucidate any one of the ſubjects propoſed, nevertheleſs I ſhall not eſteem my labour uſeleſs, or the time of the Royal Society altogether unprofitably conſumed, if I ſucceed in pointing out the way to the future attainment of knowledge ſo deeply intereſting to mankind.

The muſcle itſelf is fibrous.

The muſcular parts of animals are moſt frequently compoſed of many ſubſtances, in addition to thoſe which are purely muſcular. In this groſs ſtate, they conſtitute a flexible, compreſſible ſolid, whoſe texture is generally fibrous, the fibres being compacted into faſciculi, or bundles of various thickneſs. Theſe fibres are elaſtic during the contracted ſtate of muſcles after death, being capable of extenſion to more than one-fifth of their length, and of returning again to their former ſtate of contraction.

The enveloping membrane is elaſtic.

This elaſticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be ſafely aſſumed that the intrinſic matter of muſcle is not elaſtic.

The attraction of coheſion, in the parts of muſcle, is ſtrongeſt in the direction of the fibres, it being double that of the contrary, or tranſverſe direction.

Muſcles are irritable during life.

When muſcles are capable of reiterated contractions and relaxations, they are ſaid to be alive, or to poſſeſs irritability. This quality fits the organ for its functions. Irritability will be conſidered, throughout the preſent lecture, as a quality only.

They have leſs coheſion lengthways when dead.

When muſcles have ceaſed to be irritable, their coheſive attraction in the direction of their fibres is diminiſhed, but it remains unaltered in the tranſverſe direction.

Experiment in proof.

The hinder limbs of a frog attached to the pelvis being ſtripped of the ſkin, one of them was immerſed in water at 115° of Fahrenheit, during two minutes, when it ceaſed to be irritable. The thigh bones were broken in the middle, without injuring the muſcles, and a ſcale affixed to the ancle of each limb: a tape paſſed between the thighs was employed to ſuſpend the apparatus. Weights were gradually introduced into each ſcale, until, with five pounds avoirdupois, the dead thigh was ruptured acroſs the fleſhy bellies of its muſcles.

repeated.

The irritable thigh ſuſtained ſix pounds weight avoirdupois, and was ruptured in the ſame manner. This experiment was repeated on other frogs, where one limb had been killed by a watery ſolution of opium, and on another where eſſential oil of cherry laurel * was employed: in each experiment, the irritable limb ſuſtained a weight one-ſixth heavier than the dead limb.

The ſame doctrine confirmed by effects in the living ſubject.

It may be remarked, in confirmation of theſe experiments, that when muſcles act more powerfully, or more rapidly, than is equal to the ſtrength of the ſuſtaining parts, they do not uſually rupture their fleſhy fibres, but break their tendons, or even an intervening bone, as in the inſtances of ruptured tendo achillis, and fractured patella. Inſtances have however occurred, wherein the fleſhy bellies of muſcles have been lacerated by ſpaſmodic actions; as in tetanus the recti abdominis have been torn aſunder, and the gastrocnemii in cramps; but in thoſe examples it ſeems that either the antagoniſts produce the effect, or the over-excited parts tear the leſs excited in the ſame muſcle. From whence it may be inferred, that the attraction of coheſion in the matter of muſcle is conſiderably greater during the act of contracting, than during the paſſive ſtate of tone, or irritable quieſcence, a fact which has been always aſſumed by anatomiſts from the determinate forces which muſcles exert.

Differences obſerveable in the colour, texture, &c. of muſcular parts.

The muſcular parts of different claſſes of animals vary in colour and texture, and not unfrequently thoſe variations occur in the ſame individual.

The muſcles of fiſhes and vermes are often colourleſs, thoſe of the mammalia and birds being always red: the amphibia, the accipenſer, and ſqualus genera, have frequently both red and colourleſs muſcles in the ſame animal.

Some birds, as the black game †, have the external pectoral muſcles of a deep red colour, whilſt the internal are pale.

In texture, the faſciculi vary in thickneſs, and the reticular membrane is in ſome parts coarſe, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarſer ſpecies.

Origin of muſcle in the egg. Punctum ſaliens

An example of the origin of muſcle is preſented in the hiſtory of the incubated egg, but whether the rudiments of the punctum ſaliens be part of the cicatricula organiſed by the parent, or a ſtructure reſulting from the firſt proceſs of incubation, may be doubtful: the little evidence to be obtained on this point ſeems in favour of the former opinion; a regular

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* Diſtilled oil from the leaves of the *Prunus Lauroceraſus*.

† *Tetrao tetrix*. Lin.

confirmation of which would improve the knowledge of animal generation by ſhewing that it is gemmiferous. There are ſufficient analogies of this kind in nature, if reaſoning from analogies were proper for the preſent occaſion.

The punctum ſaliens, during its firſt actions, is not encompaſſed by any fibres diſcoverable with microſcopes, and the vaſcular ſyſtem is not then evolved, the blood flowing forwards and backwards, in the ſame veſſels. The commencement of life in animals of complex ſtructure is, from the preceding fact, like the ultimate organization of the ſimpler claſſes.

Muſcles of birds are formed out of the albumen, with a ſmall portion of vitellus and atmoſ. fluid,

It is obvious that the muſcles of birds are formed out of the albumen ovi, the vitellus, and the atmoſpheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly conſumed during incubation, and the vitellus little diminiſhed, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muſcular parts, which conſtitute the greater proportion of ſuch animals when hatched, are made out of the albumen, a ſmall portion of the vitellus, and certain elements, or ſmall quantities of the whole compound of the atmoſphere.

and they do not differ from thoſe of the mammalia.

The muſcles of birds are not different, in any reſpect, from thoſe of quadrupeds of the claſs of mammalia.

The anatomical ſtructure of muſcular fibres is generally complex, as thoſe fibres are connected with membrane, blood-veſſels, nerves, and lymphæducts; which ſeem to be only appendages of convenience to the eſſential matter of muſcle.

Muſcular fibre is cylindric; membranous without, and pulpy within.

A muſcular fibre, duly prepared by waſhing away the adhering extraneous ſubſtances, and expoſed to view in a powerful microſcope, is undoubtedly a ſolid cylinder, the covering of which is reticular membrane, and the contained part a pulpy ſubſtance irregularly granulated, and of little coheſive power when dead.

The ultimate fibres.

A difficulty has often ſubſiſted among anatomiſts concerning the ultimate fibres of muſcles; and, becauſe of their tenuity, ſome perſons have conſidered them infinitely diviſible, a poſition which may be contradicted at any time, by an hour's labour at the microſcope.

Arteries.

The arteries arboreſce copiouſly upon the reticular coat of the muſcular fibre, and in warm-blooded animals theſe veſſels are of ſufficient capacity to admit the red particles of blood, but the intrinſic matter of muſcle, contained within the ultimate cylinder, has no red particles.

The arteries of muſcles anaſtomoſe with correſponding veins; but this courſe of a continuous canal cannot be ſuppoſed to act in a direct manner upon the matter of muſcle.

The capillary arteries terminating in the muſcular fibre muſt alone effect all the changes of increaſe in the bulk, or number, of fibres, in the repleniſhment of exhauſted materials, and in the repair of injuries; ſome of theſe neceſſities may be ſuppoſed to be continually operating. It is well known, that the circulation of the blood is not eſſential to muſcular action; ſo that the mode of diſtribution of the blood veſſels, and the differences in their ſize, or number, as applied to muſcles, can only be adaptations to ſome ſpecial convenience.

Vaſcularity is not infinite, but perceptibly limited.

Another prevalent opinion among anatomiſts, is the infinite extenſion of vaſcularity, which is contradicted in a direct manner by comparative reſearches. The ſeveral parts of a quadruped are ſenſibly more or leſs vaſcular, and of different contextures; and, admitting that the varied diameter of the blood veſſels diſpoſed in each ſpecies of ſubſtance, were to be conſtituted by the groſs ſenſible differences of their larger veſſels only, yet, if the ultimate veſſels were in all caſes equally numerous, then the ſole remaining cauſe of diſſimilarity would be in the compacting of the veſſels. The vaſa vaſorum of the larger trunks furniſh no reaſon, excepting that of a looſe analogy, for the ſuppoſition of vaſa vaſorum extended without limits. Moreover, the circulating fluids of all animals are compoſed of water, which gives them fluidity, and of animalized particles of defined configuration and bulk; it follows that the veſſels through which ſuch fluids are to paſs, muſt be of ſufficient capacity for the ſize of the particles, and that ſmaller veſſels could only filtrate water devoid of ſuch animal particles: a poſition repugnant to all the known facts of the circulation of blood, and the animal economy.

Capillary arteries,

The capillary arteries which terminate in the muſcular fibre, muſt be ſecretory veſſels for depoſiting the muſcular matter, the lymphæducts ſerving to remove the ſuperfluous extravaſated watery fluids, and the decayed ſubſtances which are unfit for uſe.

and lymphæducts.

The lymphæducts are not ſo numerous as the blood veſſels, and certainly do not extend to every muſcular fibre; they appear to receive their contained fluids from the interſticial ſpaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally repreſented. This mode of receiving fluids out of a cellular ſtructure, and conveying them into cylindrical veſſels, is exemplified in the corpora cavernoſa, and corpus ſpongioſum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it paſſes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal veſſels univerſally ariſe from the looſe cellular membrane, ſituated between the internal ſpongy coat of the inteſtines and the muſcular coat. The cellular ſtructure may be filled from the lacteals, or the lacteals from the cellular cavities. When injecting the ſmaller branches of the lymphæducts retrograde in an œdematous human leg, I ſaw, very diſtinctly, three orifices of theſe veſſels terminating in the angles of the cells, into which the quickſilver trickled. The preparation is preſerved, and a drawing of the appearance made at the time. It was alſo proved, by many experiments, that neither the lymphæducts, nor the veins, have any valves in their minute branches.

Nerves of voluntary muſcles.

The nerves of voluntary muſcles ſeparate from the ſame bundles of fibrils with the nerves which are diſtributed in the ſkin, and other parts, for ſenſation; but a greater proportion of nerve is appropriated to the voluntary muſcles, than to any other ſubſtances, the organs of the ſenſes excepted.

Origin of the nerves of volition.

The nerves of volition all ariſe from the parts formed by the junction of the two great maſſes of the brain, called the Cerebrum and Cerebellum, and from the extenſion of that ſubſtance throughout the canal of the vertebræ. Another claſs of muſcles, which are not ſubject to the will, are ſupplied by peculiar nerves; they are much ſmaller, in proportion to the bulk of the parts on which they are diſtributed, than thoſe of the voluntary muſcles; they contain leſs of the white opaque medullary ſubſtance than the other nerves, and unite their fibrils, forming numerous anaſtomoſes with all the other nerves of the body, excepting thoſe appropriated to the organs of the ſenſes. There are enlargements at ſeveral of theſe junctions,

Muſcles for involuntary motion.

junctions, called Ganglions, and which are compoſed of a leſs proportion of the medullary ſubſtance, and their texture is firmer than that of ordinary nerves. Ganglions.

The terminal extremities of nerves have been uſually conſidered of unlimited extenſion; by accurate diſſection however, and the aid of magnifying glaſſes, the extreme fibrils of nerves are eaſily traced as far as their ſenſible properties, and their continuity extends. The fibrils ceaſe to be ſubdivided whilſt perfectly viſible to the naked eye, in the voluntary muſcles of large animals, and the ſpaces they occupy upon ſuperficies where they ſeem to end, leave a remarkable exceſs of parts unoccupied by thoſe fibrils. The extreme fibrils of nerves loſe their opacity, the medullary ſubſtance appears ſoft and tranſparent, the enveloping membrane becomes pellucid, and the whole fibril is deſtitute of the tenacity neceſſary to preſerve its own diſtinctneſs; it ſeems to be diffuſed and mingled with the ſubſtances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary ſenſation, appear to be in the reticular membrane, the common covering of all the different ſubſtances in an animal body, and the connecting medium of all diſſimilar parts. The extremities of nerves are definite in their termination.

By this ſimple diſpoſition, the medullary ſubſtance of nerve is ſpread through all organized, ſenſible, or motive parts, forming a continuity which is probably the occaſion of ſympathy. Peculiar nerves, ſuch as the firſt and ſecond pairs, and the portio mollis of the ſeventh, terminate in an expanſe of medullary ſubſtance which combines with other parts and membranes, ſtill keeping the ſenſible exceſs of the peculiar medullary matter. Diffuſion of nervous matter.

The peculiar ſubſtance of nerves muſt in time become ineſficient; and, as it is liable to injuries, the powers of reſtoration, and repair, are extended to that material. The re-union of nerves after their diviſion, and the reproduction after part of a nerve has been cut away, have been eſtabliſhed by deciſive experiments. Whether there is any new medullary ſubſtance employed to fill up the break, and, if ſo, whether the new ſubſtance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the hiſtory of the formation of a fœtus, the ſtructure of certain monſters, and the organization of ſimple animals, all ſeem to Reſtoration or repair of nerves.

favour the probability, that the medullary matter of nerves is formed at the parts where it is required, and not in the principal ſeat of the cerebral medulla.

Whether the matter of nerve be not extenſively mixed in all irritable parts.

This doctrine, clearly eſtabliſhed, would lead to the belief of a very extended commixture of this peculiar matter in all the ſenſible and irritable parts of animals, leaving the nerves in their limited diſtribution, the ſimple office of conveying impreſſions from the two ſentient maſſes with which their extremities are connected. The moſt ſimple animals in whom no viſible appearances of brain or nerves are to be found, and no fibrous arrangement of muſcles, may be conſidered of this deſcription: Mr. John Hunter appeared to have had ſome incomplete notions upon this ſubject, which may be gathered from his repreſentation of a materia vitæ in his Treatiſe on the Blood, &c. Perhaps it would be more proper to diſtinguiſh the peculiar matter of muſcle by ſome ſpecific term, ſuch, for example, as materia contractilis.

Peculiar adaptation for the nerves of electric animals.

A particular adaptation for the nerves which ſupply the electrical batteries of the torpedo, and gymnotus, is obſervable on the exit of each from the ſkull; over which there is a firm cartilage acting as a yoke, with a muſcle affixed to it, for the obvious purpoſe of compreſſion: ſo that a voluntary muſcle probably governs the operations of the battery.

The matter of the nerves, and brain, is very ſimilar in all the different claſſes of animals.

The external configuration of animals is not more varied than their internal ſtructure.

Configuration and ſtructure of the various claſſes of animals.

The bulk of an animal, the limitation of its exiſtence, the medium in which it lives, and the habits it is deſtined to purſue, are each, and all of them, ſo many indications of the complexity or ſimplicity of their internal ſtructure. It is notorious that the number of organs, and of members, is varied in all the different claſſes of animals; the vaſcular and nervous ſyſtems, the reſpiratory, and digeſtive organs, the parts for procreation, and the inſtruments of motion, are ſeverally varied, and adapted to the condition of the ſpecies. This modification of anatomical ſtructure is extended in the loweſt tribes of animals, until the body appears to be one homogeneous ſubſtance. The cavity for receiving the food is indifferently the internal, or external ſurface, for they may be inverted, and ſtill continue

Very ſimple animals.

tinue to digeſt food; the limbs or tantacula may be cut off, and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally ſenſible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the reſpiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilſt neither blood-veſſels, nerves, nor muſcular fibres, are diſcoverable by any of the modes of inveſtigation hitherto inſtituted.

In all animal ſtructures deſign is evident.

From this abſtract animal (if ſuch a term may be admitted) up to the human frame, the variety of acceſſory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of deſign, and of accommodations to the purpoſes which fix the order of nature.

In the more complicated animals much of their materials are not alive;

In the more complicated animals, there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of ſome liable to be diſpenſed with. The water tranſfuſed throughout the interſticial ſpaces of the animal fabric, the combinations with lime in bones, ſhells, and teeth; the horns, hoofs, ſpines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their ſubſtance, extra-vaſcular, inſenſible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extenſive obſervation, that many more parts of animal bodies may be conſidered as inanimate ſubſtances; even the reticular membrane itſelf ſeems to be of this claſs, and tendons, which may be the condenſed ſtate of it; but theſe particulars are foreign to the preſent occaſion.

ſo that animated matter may be connected with inanimate.

The deduction now to be made, and applied to the hiſtory of muſcular motion, is, that animated matter may be connected with inanimate; this is exemplified in the adheſions of the muſcles of multi-valve, and bi-valve ſhell fiſh, to the inorganic ſhell, the cancer Bernhardus to the dead ſhells of other animals, and in the tranſplantation of teeth. All of which, although ſomewhat contrary to received opinion, have certainly no degree of vaſcularity, or vital connection with the inhabitant; theſe ſhells being liable to tranſudations of cupreous ſalts and other poiſonous ſubſtances, whilſt the animal remains uninjured. A variety of proofs to the ſame effect might be adduced, but it would be diſreſpectful to to this learned body to urge any farther illuſtrations on a ſubject ſo obvious.

Diviſion of the parts of an animal deſtroys the conformation;

The effects of ſubdiviſion, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter inſtance, the entire properties of the ſubſtance are retained, however extenſive the ſubdiviſion; in the former ſubſtances, the comminution of parts deſtroys the eſſential texture and compoſition, by ſeparating the groſs arrangements of ſtructure upon which their ſpecific properties depend. From ſimilar cauſes it ſeems to ariſe, that animals of minute bulk are neceſſarily of ſimple ſtructure: ſize alone is not, however, the ſole cauſe of their ſimple organization, becauſe examples are ſufficiently numerous wherein the animal attains conſiderable bulk, and is of ſimple ſtructure, and *vice verſâ*; but, in the former, the medium in which they live, and the habits they aſſume, are ſuch as do not require extenſive appendages, whilſt the ſmaller complex animals are deſtined to more difficult, and more active exertions. It may be aſſumed however, as an invariable poſition, that the minuteſt animals are all of ſimple organization.

but leſs the more ſimple the ſtructure.

Life may on a ſmall ſcale be ſupported with ſimple materials; bulky animals require variety of organs.

Upon a ſmall ſcale, life may be carried on with ſimple materials; but the management, and proviſions for bulky animals, with numerous limbs, and variety of organs, and appendages of convenience, are not effected by ſimple apparatus; thus, the ſkeleton which gives a determinate figure to the ſpecies, ſupports its ſoft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being ſufficiently ſtrong, and yet light enough for the moving powers; but the ſkeleton is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind: in which laſt caſe the bulk is not an abſolute cauſe. The examples of teſtaceous vermes, and coleopterous, as well as moſt other inſects, are univerſally known.

Thus large animals have their bones within them, ſmaller have them without.

Cryſtalline of the eye, muſcular.

The opinion of the muſcularity of the cryſtalline lens of the eye, ſo ingeniouſly urged by a learned member of this Society, is probably well founded; as the arrangement of radiating lines of the matter of muſcle, from the centre to the circumference of the lens, and theſe compacted into angular maſſes, would produce ſpecific alterations in its figure.

This rapid ſketch of the hiſtory of muſcular ſtructure has been obtruded before the Royal Society to introduce the principal experiments, and reaſonings which are to follow: they are not ordered with ſo much exactneſs as becomes a more deliberate eſſay, but the intention already ſtated, and the limits of a lecture are offered as the apology.

Temperature of muſcles.

Temperature has an eſſential influence over the actions of muſcles, but it is not neceſſary that the ſame temperature ſhould ſubſiſt in all muſcles during their actions; neither is it eſſential that all the muſcular parts of the ſame animal ſhould be of uniform temperatures for the due performance of the motive functions.

All animals do produce heat;

It appears that all the claſſes of animals are endowed with ſome power of producing thermometrical heat, ſince it has been ſo eſtabliſhed in the amphibia, pices, vermes, and inſecta, by Mr. John Hunter; a fact which has been verified to my own experience; the term "cold-blooded" is therefore only relative. The ratio of this power is not, however, in theſe examples, ſufficient to preſerve their equable temperature in cold climates, ſo that they yield to the changes of the atmoſphere, or the medium in which they reſide, and moſt of them become torpid, approaching to the degree of freezing water. Even the mammalia, and aves, poſſeſs only a power of reſiſting certain limited degrees of cold; and their ſurfaces, as well as their limbs, being diſtant from the heart, and principal blood-veſſels, the muſcular parts ſo ſituated are ſubject to conſiderable variations in their temperature, the influence of which is known.

but are alſo affected by external communication.

The colder animals.

In thoſe claſſes of animals which have little power of generating heat, there are remarkable differences in the ſtructure of their lungs, and in the compoſition of their blood, from the mammalia and aves.

Reſpiration: its organs in animals of higher temperature;

Reſpiration is one of the known cauſes which influences the temperatures of animals: where theſe organs are extenſive, the reſpirations are performed at regular intervals, and are not governed by the will, the whole maſs of blood being expoſed to the atmoſphere in each circulation. In all ſuch animals living without the tropics, their temperature ranges above the ordinary heat of the atmoſphere, their blood contains more of the red particles than in the other claſſes, and their muſcular irritability ceaſes more rapidly after violent death.

The

and in the cold-blooded:

The reſpirations of the animals denominated "cold-blooded," are effected differently from thoſe of high temperature; in ſome of them, as the amphibia of Linnæus, the lungs receive atmoſpheric air, which is arbitrarily retained in large cells, and not alternately, and frequently changed. The fiſhes, and the teſtaceous vermes, have lungs which expoſe their blood to water, but whether the water alone, or the atmoſpheric air mingled with it, furniſh the changes in the pulmonary blood, is not known.

in inſects.

In moſt of the genera of inſects, the lungs are oboreſcent tubes containing air, which, by theſe channels, is carried to every vaſcular part of the body. Some of the vermes of the ſimpler conſtruction have no appearance of diſtinct organs, but the reſpiratory influence is nevertheleſs eſſential to their exiſtence, and it ſeems to be effected on the ſurface of the whole body.

In all the colder animals, the blood contains a ſmaller proportion of the red colouring particles than in the mammalia, and aves; the red blood is limited to certain portions of the body, and many animals have none of the red particles.

Experiment: Cold blooded animals were cluded in water over mercury After ſome days they died; but no gas was e[illegible]ted, nor was the water changed.

The following animals were put into ſeparate glaſs veſſels, each filled with a pound weight of diſtilled water, previouſly boiled to expel the air, and the veſſels inverted into quickſilver; viz. one gold fiſh, one frog, two leeches, and one freſh-water muſcle.* Theſe animals were confined for ſeveral days, and expoſed in the ſun in the day-time, during the month of January, the temperature being from 43° to 48°, but no air bubbles were produced in the veſſels, nor any ſenſible diminution of the water. The frog died on the third day, the fiſh on the fifth, the leeches on the eighth, and the freſh-water muſcle on the thirteenth. This unſucceſsful experiment was made with the hope of aſcertaining the changes produced in water by the reſpiration of aquatic animals, but the water had not undergone any chemical alteration.

Hybernating animals can live under confined reſpiration.

Animals of the claſs mammalia which hybernate, and become torpid in the winter, have at all times a power of ſubſiſting under a confined reſpiration, which would deſtroy other animals not having this peculiar habit. In all the hybernating mammalia there is a peculiar ſtructure of the heart, and its principal veins; the ſuperior cava divides into two trunks;

Peculiarity of ſtructure in the heart and its veins.

the left paſſing over the left auricle of the heart, opens into ſerior part of the right auricle, near to the entrance of the vena cava inferior. The veins uſually called azygos, accumulate into two trunks, which open each into the branch of the vena cava ſuperior, on its own ſide of the thorax. The intercoſtal arteries and veins in theſe animals are unuſually large.

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* *Mytilus Anatinus.*

Their habit and circumſtances that accompany the torpid ſtate, &c.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary ſleep, and they invariably aſſume the ſame attitude when in the torpid ſtate: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or firſt ribs, and the ſternum, are preſſed againſt the fore part of the neck, ſo as to interrupt the flow of blood which ſupplies the head, and to compreſs the trachea: the abdominal viſcera, and the hinder limbs are puſhed againſt the diaphragm, ſo as to interrupt its motions, and to impede the flow of blood through the large veſſels which penetrate it, and the longitudinal extenſion of the cavity of the thorax is entirely obſtructed. Thus a confined circulation of the blood is carried on through the heart, probably adapted to the laſt weak actions of life, and to its gradual recommencement.

The manner in which that ſtate commences and is afterwards terminated.

This diminiſhed reſpiration is the firſt ſtep into the ſtate of torpidity; a deep ſleep accompanies it; reſpiration then ceaſes altogether; the animal temperature is totally deſtroyed, coldneſs and inſenſibility take place, and finally the heart concludes its motions, and the muſcles ceaſe to be irritable. It is worthy of remark that a confined air, and a confined reſpiration, ever precede theſe phenomena: the animal retires from the open atmoſphere, his mouth and noſtrils are brought into contact with his cheſt, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant ſtate. On being rouſed, the animal yawns, the reſpirations are fluttering, the heart acts ſlowly and irregularly, he begins to ſtretch out his limbs, and proceeds in queſt of food. During this dormancy, the animal may be frozen, without the deſtruction of the muſcular irritability, and this always happens to the garden ſnail*, and to the chryſalides of many inſects during the winter of this climate.

(The concluſion in our next.)

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* *Helix nemoralis.*

Deſcription

VIII.

Deſcription of a Boring Tube, in general Uſe in America; but leſs known in this Country.

Deſcription of the American borer.

FIG. 2. *Plate* X. exhibits a very ſimple and ingenious borer, conſiſting of the common center bit of the carpenters followed by a wide flat thread ſcrew, hammered up from a plate of iron or ſteel. It is ſaid that they are uſed to bore holes ſeveral feet in length, and the peculiar property poſſeſſed by this inſtrument is, that it clears the cutting without requiring to be drawn out, as is the caſe with the augur, the gimblet, and other ſimilar tools. I do not, however, think that it would have this effect in boring perpendicularly down to conſiderable depths; but for horizontal or ſlightly inclined holes, its effect muſt fully anſwer.

Popular explanation of its action.

It may not at firſt occur to the reader why the introduction of this tool into a hole which muſt contain the wood that formerly blocked it up, ſhould not be attended with ſome degree of impediment or jamming; but this difficulty will vaniſh, when it is conſidered that the cuttings are, partly by their weight, and partly by friction againſt the internal cylindrical ſurface, prevented from revolving along with the ſcrew. The conſequence is that they are preſſed againſt its thread, and ſlide along it towards the handle. And as this motion or ſhifting of the thread is quicker than the motion of boring, by which the whole tool is carried inwards, the cuttings muſt come out with a velocity nearly equal to the difference of theſe two motions.

IX.

Geographical and Topographical Improvements. By JOHN CHURCHMAN, *Eſq. M. Imp. Acad. of Sciences, Peterſburgh.**

Great value of topographical knowledge.

IT appears to be a matter of much importance to the people of any country, at all times, whether in war or peace, to poſſeſs complete knowledge of its ſurface. In war, ſuch knowledge is abſolutely neceſſary for defence; in peace, for improving the country to the beſt advantage.

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* From the Tranſ. of the Soc. of Arts for 1804, who voted the ſilver medal to him for the ſame.

Utility of a correct delineation of hills, &c.

Now, ſince geography may be improved, an eaſy and accurate method to lay down maps of mountainous countries and hilly eſtates, will perhaps prove uſeful, as it will ſhow at a ſingle view the true ſhape and comparative height of the ground without the art of painting.

Mountains cannot be uſefully ſhewn in perſpective.

As mountains are apt to eclipſe each other, a perſpective view is ſeldom very extenſive, the rules of which fall ſhort of giving an accurate idea of any hilly country; becauſe ſuch a view, though ſtrictly true in one particular place, is not ſo in any other. The altitudes of mountains appear in proportion to the diſtance from the eye, and no rule in geometry has been found ſufficient to determine diſtances from any ſingle ſtation. Neither can a bird's-eye view of an eſtate aſcertain the depth of valleys or the height of mountains. But the method here propoſed will be found equally capable of giving the true ſhape of any ground above or below water. It may be ſucceſsfully applied to ſea charts, and will prevent much confuſion, ariſing from the tedious method of diſtinguiſhing ſoundings by a multitude of figures.

Explanation.

New method. Make a good map or plan. Mark the point of higheſt elevation and other points differing in elevation by equal meaſures. Run lines with the theodolite and level from theſe points; in which if the ſurvey be good, they will again terminate. Theſe lines laid on the map will ſhew the figure of the country.

Suppoſe a full deſcription is required of any iſland in the ocean. Firſt, let an accurate map be laid down in the common way; and let the perpendicular height between the higheſt point of land and the ocean be divided into any number of equal parts. Suppoſe theſe equal diviſions are 100, 200, 300, 400 feet above the low-water mark. From the different points of theſe ſeveral diviſions, let horizontal lines be run with a good theodolite, and ſpirit level annexed, all round the iſland. If the work is well done, each line will end where it began; and if the bearings and diſtances of theſe ſeveral lines are truly laid down on the map, the crooked courſes of them will clearly ſhow the ſhape of the ground over which they paſs. For example: if any horizontal line paſſes by the ſide of a ſteep hill, it will incline towards the ocean, or approach the next horizontal line below it. When the ſame line croſſes a ſtream of running water or a valley, it will naturally bend up the ſide of the ſaid ſtream, until it can croſs it without loſing the level; or, in other words, it will bend towards the centre of the iſland. Hence, by a little practice, the ſhape of the ſeveral horizontal lines on the map will give as clear an idea to the mind, of the ſhape of any country over which they paſs, as a ſight of the country itſelf can convey to the eye. But to obtain a mathematical and true knowledge of the altitude and declivity of any part of the country, we have the following propoſition:

Trigonometrical rule for the declivities.

As the perpendicular height of any one horizontal line above another is to the radius: ſo is the horizontal diſtance between the horizontal lines meaſured on the map at any particular place: to the co-tangent of declivity at that place.

Note.---If the horizontal diſtance between any two horizontal lines on the map is equal to the perpendicular height of any horizontal line above another, the angle of altitude, or declivity, of any hill will be 45 degrees.

Advantages;

The preſent improvement, which I believe to be entirely new, will be found to poſſeſs the following advantages:

—to military men,

1ſt. Military men are well acquainted with the many advantages always to be gained from the exact repreſentation of high grounds. By this method, we are able to give the angle of altitude, the angle of declivity, and perpendicular height of every hill; likewiſe the comparative height of different hills, the beſt route by which the high grounds may be gradually aſcended, and where heavy burthens can be drawn up with moſt eaſe.

—and for domeſtic and economical purpoſes.

2dly. Experience has ſufficiently ſhown, that the inhabitants of low grounds are ſubject to different kinds of ſickneſs, from which thoſe living at places elevated to a certain degree are exempt. A map on this improved plan will point out the moſt proper ſituation for building dwelling-houſes. It will be uſeful in botany, in diſcovering or cultivating ſome kinds of plants which flouriſh beſt at particular diſtances above the level of the ocean. It will trace the line of vegetation on the ſides of lofty mountains, whoſe tops are covered with eternal ſnow.

—to direct agricultural improvements,

3dly. Some high lands are known to produce good grain, while low lands afford graſs more abundantly; but moſt grounds produce good graſs, over which a moderate quantity of running water is conveyed. A plan of any country in this way will ſhew all the ground that can be irrigated; where water-works may be erected; where navigable canals may be cut; and where high-ways and rail-roads may be laid out on the beſt and moſt level ground.

—and mine reſearches.

4thly. The ſubterraneous treaſures of the mineral and foſſil kingdoms are generally found in ſtrata; and if they are not truly horizontal, they make a certain angle with the horizon. A map on this projection may enable the mineralogiſt to follow any one ſtratum, at places even far diſtant from each other.

Problem.

Examples computed.

To find the true declivity of any piece of ground, in any map laid down on the principles of the preſent plan.

Example 1*ſt. for* D. *ſee Plate* IV.

As the perpendicular height, 4 feet -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 4 feet	60206
	10.60206
To the co-tangent of the declivity, 45°	10.00000

Example 2*d. for* B.

As the perpendicular height, 4 feet -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 8 feet	90309
	10.90309
To the co-tangent of the declivity, 26° 34′	10.30103

Example 3*d. for* C.

As the perpendicular height, 4 feet -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 18 feet	1.25527
	11.25527
To the co-tangent of the declivity, 12° 32′	10.65321

A ſurvey according to this method.

The annexed ſurvey, *Plate* IX. of a ſmall lake and artificial mountain in the garden of his Excellency Count de Strogonoff, near St. Peterſburgh, has been cloſed by the tables of the difference of latitude and departure, as follows:

		N	S	E	
N 30 E	2½	2.2	—	1.2	
N 35 E	2	1.6	—	1.1	
N 75 E	2	.5	—	1.9	
N 55 E	2	1.1	—	1.6	
N 45 E	3	2.1	—	2.1	
N 52 W	2	1.2	—	—	1.6
N 59 W	3	1.5	—	—	2.5
S 56 W	12	—	6.7	—	9.9
S 60 E	7	—	3.5	6.1	—
		10.2	10.2	14.0	14.0

X.

A Memoir on the refining of Lead in the large Way. Containing ſome Reflections on the Inconveniencies reſulting from Cupels made of Aſhes; with a new and œconomical Method of conſtructing thoſe Cupels. By CITIZEN DUHAMEL.*

Proceſs for refining lead. Cupels made of aſhes.

EVERY one knows that in order to effect the ſeparation of ſilver from lead, a proceſs called refining or cupellation is made uſe of, which is effected in a baſon called a cupel; and it is likewiſe known that this baſon is formed of the aſhes or incinerated remains of animal or vegetable ſubſtances, after depriving them by waſhing of what ſaline matter they may contain.

The great quantity of wood aſhes required for making theſe cupels, and the difficulty of procuring it, have long ago induced me to ſeek for a more ſimple and leſs expenſive means of forming theſe veſſels.

Early obſervation that the oxide of lead penetrates chemical veſſels, while ſilver, if preſent, remains metallic.

The early chemiſts having obſerved that lead becomes oxided and converted into what is called litharge, when it is expoſed to fire with the contact of atmoſpheric air, while the ſilver it may contain, preſerves its metallic form; it only remained for them to contrive a method of ſeparating theſe two metals. They were led to this method by obſerving that the oxide of lead, in its ſtate of liquefaction eaſily penetrates the ſubſtances with which it may be in contact, particular bone aſhes, without deſtroying the figure of the veſſel made of that material. In fact there is no ſubſtance whatever which is better adapted to form the ſmall cupels for aſſaying.

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* This memoir is tranſlated from the Memoirs of the French National Inſtitute Vol. III. at the requeſt of a correſpondent.

For want of bone aſhes the aſhes of wood are uſed for cupels; they have many inconveniences.

The difficulty, and often the impoſſibility of procuring three or four buſhels of bone aſhes in Germany, for each time of refining, has led to the adoption of the aſhes of wood. But not to mention that theſe are of conſiderable price, and not always to be had, they preſent another inconvenience, by often riſing and floating on the ſurface of the lead. When this happens, the proceſs muſt fail; and it does happen as often as the aſhes are ill prepared, or the cupel not ſufficiently or irregularly beaten, or that the openings left for the evaporation of the humidity are not properly diſpoſed, or enough in number, or cloſed by a portion of the ſcoriæ upon which the floor is made to receive the aſhes. This floor ought to be conſtructed of the moſt porous bricks, in order that the water with which the aſhes muſt be wetted may penetrate them and evaporate into the bed of ſcoria, and eſcape through the opening at the bottom of the furnace.

The elaſticity of this aqueous vapour frequently cauſes exploſions, which not only diſorder the cupel, but even the maſonry of the furnace, if it be not properly conſtructed.

Cupelling of lead.

In order to aſcertain the proportion of ſilver in any quantity of lead, it is only neceſſary to paſs a few pennyweights into a ſmall cupel of bone aſhes placed under the muffle of an aſſayer's furnace; as the lead becomes oxided it is imbibed in the cupel, and at length the phenomenon of brightening takes place upon the metallic button. This appearance ſhews that all the lead is diſſipated, and the remaining ſilver in a pure ſtate.

Refining in the large way cannot be done by the aſſaying proceſs wherein the oxide is abſorbed.

In the large way of refining, the ſame object of ſeparating the ſilver from the lead is aimed at, but the lead is not intended to penetrate into the cupel; which in fact is impracticable. For the total abſorption of this metal would require a much greater quantity of aſhes, with the conſumption of a ten fold portion of time and fuel; beſides which, the loſs in recovering the lead by fuſion of the cupel is very expenſive and the product leſs conſiderable than in the common way. The oxide of lead obtained in this laſt method may be eaſily fuſed, and reduced if needful; but it is an article of value in the arts, and therefore very acceptable in the market in its ſtate of oxide.

Veſſels of clay uſed in ſmelting lead.

The lead ores and litharge may be fuſed as is done in England and Brittany, in a reverbatory furnace, of which the floor or baſon is formed of moiſtened and rammed clay. Theſe floors reſiſt the action of fire as well as that of the oxide of lead during ſix or eight months conſtant work.

Theſe are applicable to refining.

The durability of theſe baſons of earth firſt gave me a notion of the method I ſhall propoſe for refining furnaces, in which the intention is to oxide the lead in order to obtain litharge, and not to cauſe it to be totally abſorbed in the cupels, as is done when the metal is aſſayed, to ſhew how much ſilver it contains.

It would be preferable that no abſorption took place.

In the operation upon a large ſcale the cupel, though of aſhes abſorbs only part of the lead, as I have already remarked, obſerving at the ſame time that it would be much more advantageous to obtain the whole converted into litharge, of which the reduction into lead is infinitely more eaſy than that of the oxide contained in the aſhes, which reſiſt fuſion, and afford a ſcoria that always contains ſome metal.

Engliſh proceſs. 12 tons of lead are oxided with ſcarcely any abſorption.

Upon a cupel of aſhes rammed into an oval ring of iron, about five feet in length and three and a half in width, the Engliſh refine in ſucceſſion about twelve ton of lead, which becomes converted into fine merchantable litharge, with the exception of the ſmall portion that penetrates the cupel, of which the thickneſs is leſs than three inches. This cupel is ſupported under the roof of the furnace by two bars of iron. The litharge is driven by the blaſt of bellows towards the anterior part of the furnace, whence it falls without interruption upon the area of the foundery; and at the ſame time to ſupply the ſpace which this ſubtraction of oxide would leave, a pig of lead is gradually advanced into the interior of the furnace, placed on one ſide of the nozel of the bellows. This lead, by its gradual fuſion, keeps the cupel full till towards the end of the operation.

The veſſel is very cheap.

I have given this ſketch of the Engliſh proceſs only to ſhew that it is poſſible to refine lead with very little expence of aſhes for forming the cupels. Thoſe here mentioned do not abſorb eighty pounds of oxide out of the large quantity of lead thus refined.

Hence we ſee that metallurgiſts have always endeavoured to obtain the greateſt poſſible quantity of litharge and the leaſt of aſhes containing oxide; but as they did not imagine they could depart from the docimaſtic proceſs, they have conſtantly made their cupels of aſhes.

The litharge runs off in the ſmall proceſs;

We have ſeen that in the ſmall proceſs of cupellation the lead penetrates the aſhes as it becomes oxided, and that when no more lead remains the ſmall button of ſilver remains pure at the bottom in the ſpherical form. This operation is effected with more ſpeed becauſe the ſurface of the bath is always convex in theſe ſmall veſſels, and conſequently the litharge runs off on all ſides towards the edge of the cupel, where it is immediately imbibed.

—but cannot in the large.

This is not the caſe in large cupels of ſeveral yards diameter. Bellows muſt be uſed, not only to accelerate the oxidation by their blaſt, but to drive the litharge towards the paſſage or gutter which is left for its iſſue.

It muſt be driven off by bellows.

The inconveniencies and even the impoſſibility of cauſing all the lead to penetrate the aſhes of theſe large cupels have been already remarked. This muſt be evident on reflecting that the oxidation can take place only at thoſe parts of the bath which are expoſed to the contact of the air and the blaſt of the bellows. The litharge near the middle of the baſon not being diſpoſed to flow towards the edge, would cover and defend the metal from any farther oxidation. Hence it is that the operators have found themſelves obliged to drive out the litharge by the mechanical action of a ſtream of air from bellows.

The oxidation is effected only at the ſurface.

The oxidation therefore takes place only at the ſurface of the lead, and not lower; if it were otherwiſe, the aſhes of the cupel would be penetrated with oxide to a depth which would be more unequal the longer the operation laſted. Now I have always remarked that the portion of aſhes thus imbibed in the large refinery is not thicker towards the center of the baſon than towards its edge, though the lead remains thirty or forty times longer at the bottom than near the edge, becauſe the bath conſtantly diminiſhes till all the lead is reduced into litharge, and nothing at laſt remains but the button of ſilver at the bottom of the cupel.

Other reaſons why the large proceſs cannot be made by abſorption.

That the whole of the lead is abſorbed in the cupel of aſſay, ariſes from the unequal action of heat upon every part of the ſmall veſſel. As the cupel in the large way preſents only its ſuperior ſurface to the action of the heat, the oxide thus imbibed ceaſes to penetrate at the place where the temperature is no longer in a ſtate to hold the oxiding fuſion. For this reaſon it is that the whole of the cupel is throughout impregnated to an equal depth, and it is impoſſible to cauſe all the lead to penetrate the aſhes.

Litharge is ſought as a product in the large way

From the preceding obſervations it will be eaſy to conclude that though the aſſay of lead muſt be made in ſmall cupels of bone aſhes, in order that the whole of the oxided metal may either be abſorbed or partly evaporated; yet the caſe is very different in the large operation, where the object is to proceed with celerity, and to obtain as much litharge as poſſible.

Addition of ſand to the wood aſhes, &c.

I have before ſtated that the wood aſhes uſed in forming large cupels are expenſive, and frequently not to be procured in ſufficient quantity; to which I have added their being ſubject to blow up or riſe entirely, which occaſions a conſiderable loſs. It muſt further be mentioned, that in order to give more weight and conſiſtence to theſe cupels it is often neceſſary to mix a conſiderable quantity of ſand with them, particularly if the lead ſhould contain foreign ſubſtances, ſuch as arſenic, cobalt, antimony, zinc, tin and other matters. If the lead be merely arſenical, after having taken off the firſt ſcum, it is uſual to throw from time to time, on the whole ſurface of the bath, about 20lbs. of iron ſcales or granulated crude iron. This iron being lighter than the lead, floats on the top and abſorbs the arſenic, after which it is cleared away, and then the litharge is formed without any obſtacle. This method is uſed in Saxony.

The neceſſity of adding ſand to the aſhes of the cupels ought to have led to a diſcovery which I propoſe: it is as follows.

New Conſtruction of the Cupels or Baſons for refining Lead.

New conſtruction of the baſons or veſſels for refining lead.

Without making any change in the maſonry of the furnaces for refining by what we call the German method, it is only neceſſary to be careful to make a ſufficient number of vents in their baſe for the evaporation of the moiſture, and to diſpoſe of them ſo as moſt effectually to anſwer this purpoſe. Theſe channels or vents are to be covered with a bed of ſcoria, upon which a pavement is to be made of the moſt porous bricks, and of the thickneſs of a ſingle brick.

On this area or pavement, which ought to be concave like the baſe upon which the aſhes of the ordinary cupels are placed, muſt be laid a quantity of founder's ſand, a little moiſtened. If it be not adheſive enough, a little clay may be added, in order to give the requiſite ſolidity, and the whole carefully mixed. The ſand muſt be rammed down in the ſame manner as is done to conſolidate the aſhes in the uſual way, and a baſon for refining muſt be formed, equally rammed in all its parts. The thickneſs of this cupel ſhould be about ſix inches; and it may be made in two layers, as we ſhall hereafter obſerve.

The cupel is made of ſand with a little c instead of bone aſhes.

After the baſon has been in all parts uniformly beaten or rammed down, it will be proper to ſift over the whole ſurface two or three quarts of wood aſhes, which may be made to adhere by ramming.

When the cupel is thus prepared, the head of the furnace muſt be lowered, and a moderate fire kept up for ſeveral hours, in order to evaporate part of the water from the ſand. The reſt will be driven out, without inconvenience, through the vents during the refining.

It may be dried before uſe;

After a ſufficient drying, which may even be diſpenſed with; the head is to be raiſed, the cupel, ſuffered to cool a little, and ſtraw or hay then laid upon it, and upon this the pigs or pieces of lead, which are to be gently put down, in order that their weight may not make impreſſions in the ſand. The ſtraw is uſed for this purpoſe, in our method as well as the common method; and it would be convenient that the lead ſhould be caſt in iron hemiſpherical moulds or pots inſtead of the priſmatic form, as theſe pieces would be leſs ſubject to damage the cupel.

—but this is not abſolutely neceſſary.

Method of charging with lead.

When the quantity of lead neceſſary to fill the cupel is arranged in the furnace, the head is to be lowered and luted all round with clay, after which the fire is to be applied as in the uſual proceſſes.

When the lead is in perfect fuſion, and the bath covered with droſs and coally matter from the ſtraw, the ſkum muſt be raked

Fuſion.

Clearing of the ſurface.

raked off through the paſſage left for the litharge, by means of a wooden rake about a foot long, with an iron handle of ſufficient length to reach every part of the bath.

Method of blowing, &c.

When the lead has been ſeveral times ſkimmed, and begins to become red, the bellows muſt be ſet into action, gently at firſt, and afterwards more ſtrongly. Their nozles muſt be ſo diſpoſed, that the blaſt may be directed towards the center of the bath, and in order that the wind may be urged upon the ſurface of the metal, each nozle muſt have a ſmall round plate of iron adapted to it. Theſe ſmall flaps or valves, called papillons (flies or butterflies) are uſed in the German fineries. They have an hinge at top, and at every ſtroke they riſe about half way from their perpendicular poſition towards the level, ſo that by reflecting the air downwards upon the lead, they haſten its oxidation.

Gutter for the litharge.

When all the droſs has been removed, and the lead is of a good red heat and covered with litharge, a little gutter muſt be made, with an hook appropriated for this purpoſe, in the ſand of the cupel. This muſt be carefully done until the bottom of the gutter anſwers to the level of the bath. The litharge driven by the blaſt of the bellows will flow out of this paſſage, and fall upon the hearth of the foundery.

Inſtructions.

When the operator perceives that only a ſmall quantity of litharge remains near the gutter, he will ſtop its eſcape with a ſmall quantity of moiſtened aſhes; but as ſoon as the lead ſhall be again covered with oxide, the paſſage muſt be opened and made deeper as the quantity of matter becomes depreſſed; taking care that no lead eſcapes, particularly towards the end of the operation, as it would carry along with it a large portion of ſilver which would be loſt.

In this manner the proceſs is to be carried on until the ſurface of the ſilver exhibits, thoſe flaſhes which are called the brightening, taking care to raiſe the fire in proportion to the diminution of the bath, particularly towards the end, when the ſilver is collected; and as this metal is much more difficult to be kept in fuſion than its ſmall alloy of lead, the refining will be imperfect, unleſs the temperature be raiſed, and inſtead of about one twentieth of lead, which the ſilver uſually retains in the German method, it will remain much more highly charged. This would render it more difficult to be treated in the ſecond operation, called the ſilver refining, or by the Germans *ſilver brenen*, by which it is rendered pure.

Thoſe who are accuſtomed to refine lead in the German method, will find no difficulty in following mine. For though the cupel is made of ſand inſtead of aſhes, there is no difference in the manipulations.

This method admits of keeping up the charge during work,

We have ſeen that the Engliſh refine a great quantity of lead on a ſmall cupel. The ſame may be done in the method here deſcribed, by adding in proportion as the loſs by oxidation takes place. Suppoſing the capacity of the cupel to be ſuch as to contain about five ton of lead, we might continue the operation to three times that quantity in a ſingle proceſs, which would not have the inconveniences of the Engliſh method.

and the cupel may be repeatedly uſed.

I flatter myſelf that a well-made cupel of ſand may be uſed for ſeveral refinings, without requiring to be made up again every time like thoſe of aſhes; but in theſe circumſtances, and before the lead is put in, it is neceſſary to fill with well beaten ſand the gutter or opening which was before made for carrying off the litharge. Care muſt be taken in doing this to remove with a chiſſel that kind of glaze which the oxide of the lead leaves behind it. With this precaution, after wetting the part where the new moiſtened ſand is to be applied, they will firmly unite together.

From the long duration of the earthen floors of thoſe reverberatory furnaces in which lead ores and even litharge are fuſed, as was before mentioned, we have no reaſon to apprehend any bad conſequences from the oxide of lead, which acts only on the ſurface of the cupel, and penetrates to an inconſiderable depth.

Recovery of the abſorbed lead.

After one or two refinings, this cruſt of oxide may be taken off and fuſed in a blaſt furnace, in order to recover the lead; a proceſs no leſs eaſy than that of reducing the metal which exiſts in much greater quantity in the ordinary cupels. We therefore obtain a larger quantity of litharge, which is one advantage, and in addition to this, the loſs in ſilver which accompanies the abſorbed lead will be leſs. For in the ſmall quantity of precious metal which accompanies the oxide, it is found by experiment, that the proportion is greater in the abſorbed lead than in that which is driven over in litharge.

To make the cupel of clay inſtead of ſand would be more expenſive and leſs convenient.

Inſtead of ſand we might make uſe of clay in conſtructing our cupels, as is done in the hearths of the reverbatory furnaces of Brittany; but it would then be neceſſary to pound the earth repeatedly for ſeveral days, otherwiſe it would crack, and theſe cracks, which would become wider by the ſhrinking from heat, would afford a lodgment for ſome of the lead; an inconvenience which ſand, even if rather loamy, does not preſent. It muſt alſo be remarked, that a cupel of clay would become too hard to admit of the excavation for carrying off the litharge; ſo that this part at leaſt would require to be made of ſand or aſhes.

Two kinds of ſand.

It will be advantageous to uſe two kinds of ſand in forming a baſon of the cupel, the one fine ſuch as the founders' ſand, and the other coarſe. The latter may form the firſt ſtratum, which, after being well rammed with the implements uſed for this purpoſe, muſt be left about three inches in thickneſs. Upon this the fine and ſomewhat loamy ſand is to be ſpread and rammed like the firſt. A ſlight degree of moiſture muſt be uſed with both theſe, in order that they may more ſolidly adhere together. The lower ſtratum being more coarſe, will facilitate the eſcape of the humidity,

Repair or renewal of the cupel.

It will not be neceſſary to diſturb the lower ſtratum of ſand when a new cupel is to be made; and even of this laſt that portion which has not imbibed any oxide may be uſed along with the new ſand intended to be applied. The lower ſtratum muſt not be touched during this renewal, for fear of mixing coarſe ſand with the fine. This inconvenience may be guarded againſt by ramming upon the ſurface of the coarſe ſand a bed of a thin facing of aſhes, at which the operator muſt ſtop when he takes away the upper ſtratum.

We have remarked that the founders' ſand muſt be rather loamy, and that if it be not ſo, it will be neceſſary to add a ſmall quantity of clay to render it adheſive; but as it is neceſſary that this clay ſhould be equally diffuſed through the maſs, it may be diffuſed in the water with which the ſand is to be ſprinkled, and the whole muſt be carefully mixed.

Abſorption is of no advantage in the old proceſs.

It might be objected, that ſince the cupels of ſand do not abſorb ſo much litharge as thoſe of aſhes, more time will be required to complete the refining, becauſe the oxide inſtead of being in part abſorbed, muſt by this new proceſs be driven out of the furnace. This however is a ſubject which ought not to be conſidered as of any importance; for the blaſt of the bellows well directed will cauſe the oxide of litharge to flow out more abundantly through the gutter than if the abſorption took place.

I have

Improvement. Cavity to receive the ſilver.

I have ſeen operators in Germany, who, when they conſtructed their cupels, had the precaution to form a ſmall circular cavity in the middle, the depth of which was proportioned to the quantity of ſilver, which from the aſſay they knew to be contained in the lead of one operation. By this contrivance there were no inſulated grains of the metal left on the ſurface of their cupel, but the whole of the ſilver formed a perfectly round cake in the middle of the center excavation. I would adviſe the ſame ingenious expedient to be uſed in the cupels of ſand.

Concluſion.

I am well aſſured that the cupels I propoſe, if made with care and attention, will ſucceed perfectly, and that, independent of their convenience beyond the others, they will be found very economical. I am deſirous that, for the advantage of metallurgy, this method ſhould be generally uſed, and its benefits will prove that we ought not always to follow with ſervility the eſtabliſhed uſages nor the common working proceſſes.

XI.

Letter from Mr. JAMES STODART, *explaining the Method of gilding upon Steel by Immerſion in a Liquid, which has lately engaged the public Attention in various Articles of Manufacture.*

To Mr. NICHOLSON.

DEAR SIR,

Inſtruments of ſteel gilt with gold.

A Conſiderable degree of public intereſt and curioſity has lately been excited by the exhibition of inſtruments of ſteel coated or gilt with gold. The diſcovery, although not altogether new, does not appear to be very generally known; and as its application to various manufactures promiſes to be both elegant and uſeful, I have, with a view of ſaving ſome time and expence to others who may be inclined to make further experiments, added a ſhort account of a method which with me has ſucceeded perfectly well. I wiſh here in juſtice to obſerve, that this diſcovery of the method belongs more to my friend Mr. Hume, chemiſt, Long-Acre, than to myſelf. With that gentleman's kind aſſiſtance I had but few difficulties

to

Account of the proceſs. Three parts ſulphuric ether are added to one of ſolution of gold. The gold is taken up by the ether.

to overcome. The following is our method: To a ſaturated ſolution of gold in nitro-muriatic acid, add about three times the quantity of pure ſulphuric ether: Agitate them together for a ſhort time. The gold will ſoon be taken up by the ether in the form of a muriate, or nitro-muriate of gold, leaving the remaining acid colourleſs at the bottom of the veſſel, which muſt now be drawn off by means of a ſtop-cock, or other

The inſtrument to be dipped into the ethereal ſolution.

ſimilar contrivance. The acid being diſcharged, the inſtrument to be gilt having been previouſly well poliſhed and wiped very clean, is to be dipped for an inſtant into the ethereal ſolution, and on withdrawing it, is inſtantly waſhed by agitation in clean water. This is eſſential to get clear of a ſmall portion of acid neceſſarily taken up with the metal; and if this be neatly done, the ſurface of the ſteel will be completely and very beautifully covered with gold. Some little degree of dexterity is required to perform the whole operation well.

Eſſential oils do not ſucceed well.

I have tried ſome of the eſſential oils, knowing that they will take the gold from nitro-muriatic acid; but as far as I went they did not apply for the purpoſe of gilding: and as I had found all I wanted in ether, I certainly did not proſecute the other experiments with much induſtry.

I remain, with much reſpect,

Dear Sir,

Your obliged ſervant,

J. STODART.

Strand, June 24, 1805.

XII.

On the peculiar Noiſe emitted by Water before it acquires the Temperature of boiling; which is commonly denoted by the Word Simmering. W. N.

Introduction.

SOME time ago a philoſophical friend who favoured me with a viſit, mentioned in converſation that the ſimmering of water before it boils had formed the ſubject of enquiry between himſelf and other curious examiners of natural appearances, as being a fact not yet well explained. It ſeems to me very remarkable, as I dare ſay it will to my readers, that any of the facts relating to the boiling of water, concerning which ſo much has been ſaid and written, ſhould ſtill remain in obſcurity. Having, myſelf, been in the habit of conſidering it as the conſequence of a rapid eſcape of interſperſed air from the heated water, I requeſted he would ſay why he thought the phenomenon repugnant to that ſuppoſition. My friend proceeded in reply to ſtate, that when water is firſt put on the fire, in a metallic veſſel, the veſſel itſelf gradually becomes lined with bubbles; that theſe bubbles become detached and riſe, ſo that the whole ſurface, or at leaſt the bottom, becomes clear again; that, ſoon afterwards a rattling noiſe is heard, ſharp and metallic, which encreaſes in loudneſs until it almoſt perfectly reſembles the pouring of ſmall ſhot into the veſſel; that the fluid continues in a ſtate of tranquillity and tranſparency during this ſtate; and laſtly, that when the noiſe is loudeſt the ſtate of ebullition ſuddenly comes on, the peculiar noiſe of ſimmering ceaſes all at once, and nothing is heard but the ſoft and moderate noiſe of aquatic agitation as long as the boiling laſts.

Particular deſcription of the act of ſimmering.

This account, ſo remarkable for its preciſion and accuracy, ſhewed clearly that my notion, which I believe is the common opinion, was ill founded. After a little meditation, it appeared evident to me that the noiſe of ſimmering muſt ariſe from the collapſion of ſteam bubbles, formed at the bottom of the veſſel, and condenſed almoſt inſtantly upon their aſcent in the fluid not yet heated to the boiling point. In ſupport of this opinion I ſhewed him a common experiment with the water-hammer. This inſtrument, which is made and ſold by the glaſs-blowers and barometer-makers, conſiſts of a tube, nearly a foot in length and about three quarters of an inch in diameter, terminating in a globe of about two inches in diameter; the other end of the tube being cloſed. The outer extremity of the globe ends in a capillary tube, through which as much water has been introduced as rather more than fills the globe itſelf. This water has been boiled in the veſſel or inſtrument, and at the time of boiling, when all the internal cavity not containing water was filled with ſteam to the almoſt total excluſion of air, the capillary aperture was hermetically cloſed. The inſtrument thus completed is found when cold to contain water and a ſpace nearly vacuous, and the experiment from which it derives its name of

It ſeems to ariſe from the ſudden production and condenſation of ſteam.

Deſcription of the water-hammer.

Singular noiſe of water agitated in vacuo.

of the water-hammer, is that of agitating its contents. The remarkable effect it exhibits is, that the parts of the water ſtrike againſt the glaſs and againſt each other, with the ſharp noiſe uſually produced by the colliſion of hard bodies. Another experiment, which I alluded to, is that, if the ball be held downwards ſo as to become filled with the water, and it be then ſlowly raiſed up, ſo as to bring the tube nearer to the horizontal poſition, the heat of the hand which holds the tube will produce vapour or ſteam ſtrong enough to prevent the fluid from running down to its level from the upper ſurface of the globe; but a little farther inclination cauſes it to deſcend, and a bubble of the ſteam enters the globe itſelf and aſcends through the fluid. But it ſcarcely arrives at the top before it is condenſed, and the water collapſes with a ſmart ſtroke or noiſe, ſo as to fill the globe again. The ſucceſſion of theſe bubbles and their condenſation take place ſo rapidly, in a well made inſtrument, that ten or twelve collapſions occur in every ſecond of time. The fact, and the obvious remarks I made upon it, convinced my friend that I had ſuggeſted the proper explanation.

Another experiment of ſteam produced and condenſed twelve times in a ſecond,

applied to explain the effect of ſimmering.

Objections.

It was my intention however to have heated ſome water in a veſſel in order to obſerve and aſcertain the progreſſive appearances, but I had not done it when I again had the pleaſure to meet this intelligent obſerver. He complimented me upon the ingenuity of my ſolution, but having himſelf ſince repeated the experiment of boiling water, it ſeemed from his report that ingenuity was all the value it could claim. "Take a bright tin veſſel" ſaid he "and heat water in it; you will hear the noiſe, but no bubbles are to be ſeen."

I took the earlieſt opportunity of making ſome experiments, the particulars and reſults of which are as follow:

Exp. 1. Water was heated in a glaſs retort. The ſimmering was attended with ſteam bubbles.

Exp. 1. A ſmall glaſs retort, the body of which is about two inches in its ſhorteſt or horizontal diameter, was ſuſpended ſo that its neck was elevated about twenty degrees above the level. Water was then poured in to fill the body and the greateſt part of the neck. My intention in filling the neck was, that I might be able to obſerve whether any greater or more ſudden riſe took place before the period of boiling than the well known expanſion of the fluid. A ſmall ſpirit-lamp was placed beneath the bulb. The coldneſs of the water in the veſſel immediately condenſed a portion of the water which

[illegible]

iſſued from the flame itſelf, and formed drops on the outſide. As the included water became heated this condenſed water evaporated, and left the ſurface again clear; and at this period the diſſeminated air began to ſeparate and gave a duſty appearance to the inner ſurface, which laſted about three minutes. At the end of this time the inner ſurface began to clear; the peculiar noiſe of ſimmering was heard; and bubbles were ſeen ſuddenly appearing and collapſing; the retort itſelf being agitated and the ſurface of the water riſing and falling by ſtarts. The bubbles were pointed at top, ſomewhat reſembling ſmall flames ſuddenly appearing and vaniſhing at different parts of the ſurface. In the courſe of one minute they grew larger and larger and collapſed at greater heights, until at length they eſcaped through the fluid without being condenſed. This was the inſtant of ebullition or boiling and at this period the noiſe of ſimmering ceaſed and that of boiling was heard.

Exp. 2. Water was heated in a larger glaſs veſſel. The effects were rather more evident.

Exp. 2. As this effect appears to ariſe decidedly from the upper water being colder than that near the bottom of the veſſel, it was natural to infer that the appearances would be different according to its figure and magnitude. I therefore took a bolt-head, or ſpherical glaſs body, with a ſtraight neck: Its diameter was four inches; and when it was filled, a column of water eight inches long ſtood in its upright neck. The thickneſs of the glaſs at its bottom was conſiderable. At 35 minutes after three the lamp was lighted. At 40 minutes bubbles of gas roſe ſingly, and very little of the duſty appearance was ſeen. At 58 minutes the noiſe of ſimmering began, and the collapſing bubbles were plentiful and diſtinct. Little ſtreams or fountains of ſteam roſe from particular points and were condenſed; and ſome globes of half an inch diameter aſcended clear of the bottom and collapſed in the fluid above. At one minute after four the bubbles reached the top of the fluid without collapſing, and at this time the noiſe of ſimmering ceaſed. The lamp was then blown out.

Exp. 4. The experim. it was repeated in a copper veſſel.

Exp. 4. A bright copper hemiſphere, four inches in diameter, was filled with water at the temperature of 60 degrees. At four hours three minutes the lamp was lighted and water became condenſed on the outſide. At five minutes the inſide ſurface had a duſty appearance, from bubbles of air immediately over the flame. The temperature was then 70°, and the outſide dry. At ſix minutes bubbles of gas or air were detached and roſe, the temperature being 120°, and the vapour of ſteam being viſible from the ſurface of the water. At eight minutes the inſide ſurface was coated with large bubbles or beads of air; temperature 150°. At nine minutes, temp. 165°, much vapour. At 9½ min. temp. 175°, the bottom was clear of bubbles, and the noiſe of ſimmering began. At 10 min. temp. 184°, the ſteam collapſing bubbles were very evident, though, from their pointed ſhape, they were not immediately obvious to an obſerver looking ſtraight down into the ſhining veſſel. At 11 min. temp. [illegible], noiſe very loud and bubbles more and larger. At 12 min. temp. 185°, ſome of the bubbles broke at the ſurface, and the noiſe was leſs. At 12½ min. temp. 201°, the water boiled, and the ſimmering noiſe was ſucceeded by that of boiling. At 14 min. the lamp was extinguiſhed.

Exp. 5. Water already heated does not ſimmer ſo much or ſo loudly as water quickly raiſed from a low temperature.

Exp. 5. The water was ſuffered to cool to 170°, and the lamp was then lighted again, namely, at four hours 18 minutes. At 20 min. temperature 180°, the ſimmering began; but not till after the ſteam bubbles were ſeen very large: and ſoon afterwards, at 204°, they roſe through the fluid, and the boiling took place by fountains or ſtreams of bubbles riſing from particular points. The ſimmering noiſe in this experiment was much leſs than before.

The thermometer was placed horizontally, with the greateſt part of its tube, and part of its bulb, out of the water. When the bulb was plunged in the water, it ſhewed 208°.

Concluſion.

From theſe facts it appears to be clearly eſtabliſhed, that the cauſe firſt mentioned, namely, the condenſation of ſteam bubbles in their aſcent through the cold fluid above, is the occaſion of the noiſe of ſimmering. In the fifth experiment the ſuperincumbent water was hotter than that beneath, and conſequently the ſimmering could not be produced but by ſteam at a higher temperature, and even then the collapſion of the water was leſs ſudden and the noiſe leſs loud.

XIII.

Experiments on Wootz. *By Mr.* DAVID MUSHET. *From the Philoſophical Tranſactions,* 1805.

Experiments on five cakes of wootz.

THE following experiments were made at the requeſt of Sir Joſeph Banks, on five cakes of wootz, with which he ſupplied me for that purpoſe. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the ſame quality, it will be proper firſt to deſcribe the differences obſervable in their external form and appearance.

Deſcription of the cake, No. 1.

No. 1 was a denſe ſolid cake, without any flaw or fungous appearance upon the flat, or, what I ſuppoſe to be, the upper ſide. The round or under ſurface was covered with ſmall pits or hollows, two of which were of conſiderable depth; one through which the ſlit or cut had run, and another nearly as large towards the edge of the cake. Theſe depreſſions, the effects, as I ſuppoſe, of a ſpecies of cryſtallization in cooling, were continued round the edges, and even approached a little way upon the upper ſurface of the wootz.

Fractured very ſteely.

The cake was a quarter of an inch thicker at one extremity of the diameter than at the other, from which I infer, that the pot or crucible, in which this cake had been made, had not occupied the furnace in a vertical poſition. Its convexity, compared to that of the other five, was ſecond. Upon breaking the thin fin of ſteel, which connects the half cakes together, I found it to poſſeſs a very ſmall denſe white grain. This appearance never takes place but with ſteel of the beſt quality, and is leſs frequent in very high ſteel, though the quality be otherwiſe good.

Upon examining the break with attention, I perceived ſeveral laminæ and minute cells filled with ruſt, which in working are never expected to unite or ſhut together. The grain otherwiſe was uniformly regular in point of colour and ſize, and poſſeſſed a favourable appearance of ſteel.

Deſcription of the cake of wootz No. 2.

No. 2. This cake had two very different aſpects; one ſide was denſe and regular, the other hollow, ſpongy, and protuberant. The under ſurface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one ſide, it becoming flatter. The grain expoſed by breaking was larger, bluer in colour, and more ſparkling than No. 1. In breaking, the fracture tore but ſlightly out, and diſplayed the ſame unconnected laminæ with ruſty ſurfaces, as was obſerved in No. 1. Beſide theſe, two thin fins of malleable iron projected from the unſound ſide, and ſeemed incorporated with the maſs of ſteel throughout. Towards the centre of the break, and near to the excreſcence common to all the cakes, groups of malleable grains were diſtinctly viſible. The ſame appearance, though in a ſlighter degree, manifeſted itſelf in various places throughout the break.

Cake of wootz,
No. 3.

No. 3. The upper ſurface of this cake contained ſeveral deep pits, which ſeemed to reſult from the want of proper fluidity in fuſion. They differed materially from thoſe deſcribed upon the convex ſides of No. 1 and 2, and were of that kind that would materially effect the ſteel in forging

The under or convex ſide of this cake preſented a few cryſtalline depreſſions, and thoſe very ſmall; the convexity was greater than that of No. 1 and 2, the fracture of the fin almoſt ſmooth, and only in one place exhibited a ſmall degree of tenacity in the act of parting. In the middle of the break, about half an inch of ſoft ſteel was evident; and in different ſpots throughout numerous groups of malleable grains, and thin laminæ of ſoft blue tough iron made their appearance.

Cake of wootz,
No. 4.

No. 4. Was a thick denſe cake poſſeſſed of the greateſt convexity; the depreſſions upon the under ſide were neither ſo large, nor ſo numerous as thoſe in No 1 and 2, nor did they approach the upper ſurface of the cake further than the acute edge. This ſurface had the moſt evident marks of hammering to depreſs the feeder, or fungous part of the metal, which in the manufacturing ſeems the gate or orifice by which the metal deſcends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being ſolid. Towards the feeder it ſeemed looſe and crumbly, and much oxidated. The grain divided itſelf into two diſtinct ſtrata, one of a denſe whitiſh colour, the other large and bluiſh, containing a number of ſmall ſpecks of great brilliancy. Several irregular [illegible] of malleable iron pervaded the maſs in various places, which indicated a compound too heterogeneous for good ſteel.

Cake of wootz,
No. 5.

5th cake. This was materially different in appearance from any of the former. It had received but little h [illegible] yet was ſmooth and free from depreſſions, or honey-comb on both ſurfaces. The feeder, inſtead of being an excreſcence, preſented a deep concave beautifully cryſtallized.

In breaking, the fracture tore out conſiderably, but preſented a very irregular quality of grain. That towards the under ſurface was ſmall and uniform, but towards the flat or upper ſurface it increaſed in ſize, and in the blueneſs of its colour, till it paſſed into the ſtate of malleable iron.

The break of this ſteel, though apparently ſoft, was the leaſt homogeneous of the whole, and throughout it preſented a very brilliant arrangement of cryſtal, which in other ſteel is always viewed with ſuſpicion.

General Remark.

Uniformly the grain and denſity of the wootz are homogeneous, and free from malleable iron towards the under or round ſurface; but always the reverſe towards the feeder or upper ſide.

Remarks in Forging.

The appearances on forging the cake of wootz, No. 1.

No. 1. One-half of the cake was heated ſlowly by an annealing heat to a deep red, and put under a ſharp broad-mouthed chiſſel with a ſmall degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the ſlit or cut originally in the cake.

The heat in this trial was ſo moderate, that I was afraid that the crack had ariſen from a want of tenacity, occaſioned by the heat being too low.

The other half was heated a few ſhades higher, and ſubjected to the ſame mode of cutting; before the chiſſel had half way reached the bottom, the piece parted in two in the direction of the depreſſion made by the cutting inſtrument. The additional heat in this inſtance proved an injury, while the cracking of the ſteel in both caſes, particularly the former, was a certain proof of the abundance, or rather of the exceſs of the ſteely principle.

The fractures of both half cakes, now obtained for a ſecond time, were materially different from that obtained by the ſimple diviſion of the cake. The grain was nearly uniform, diſtinctly marked, but of too gray a colour for ſerviceable ſteel. Two of the quarters being drawn into neat bars under hand ham-

The appearances on forging the cake of wootz, No. 1.

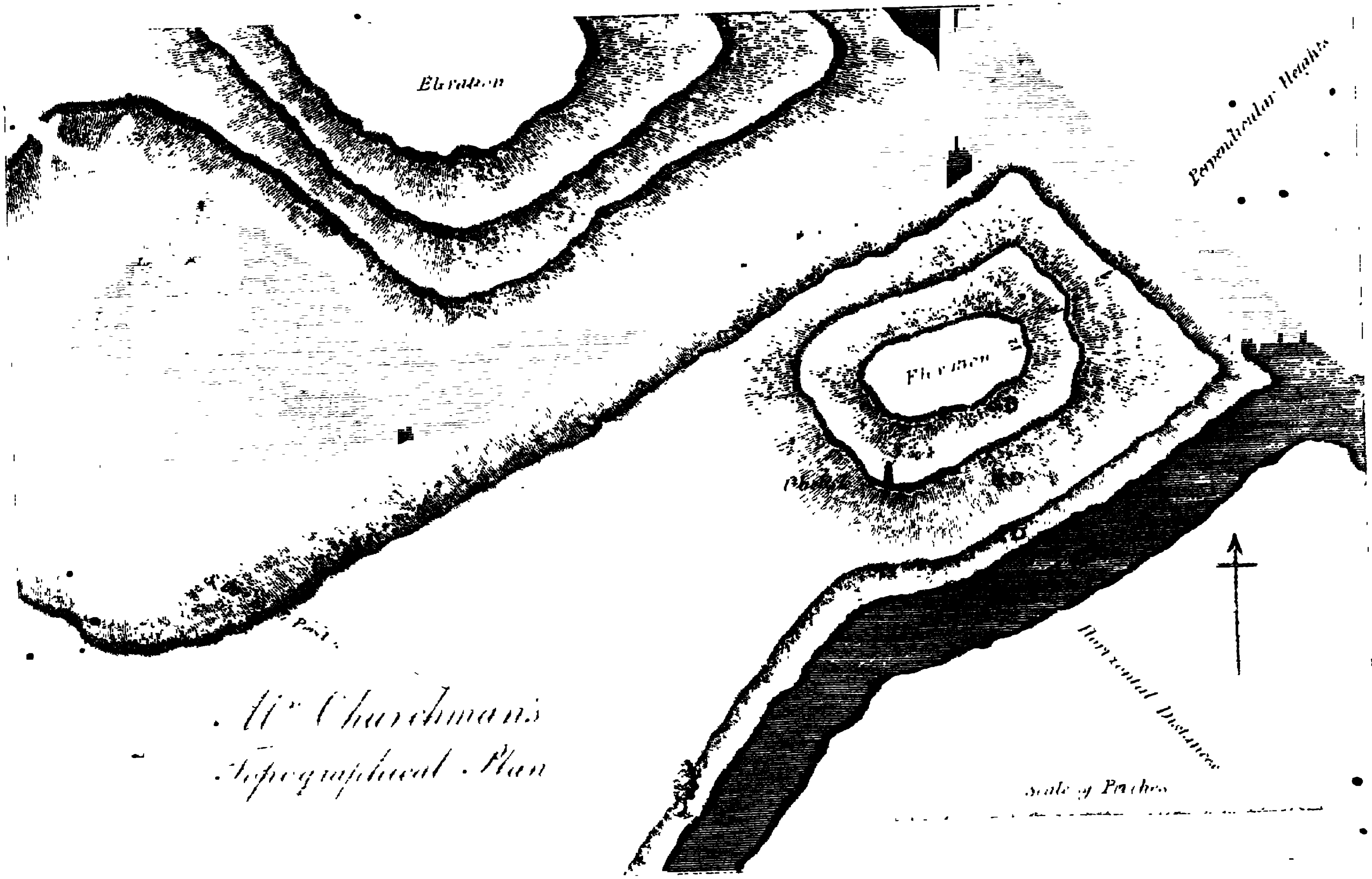
mers at a low heat, one of them contained a number of cracks and fiſſures. The fracture was gray, tore out a little in breaking, but was otherwiſe ſilky and exceſſively denſe. A ſmall bar of penknife ſize was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the ſteel was greater, and it ſtood firmly under the hammer at a bright red heat.

The other two quarters of this cake were ſquared a little, and ſucceſſively put under a tilt hämmer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into ſmall penknife ſize. One of the bars from an outſide piece, always the moſt ſolid, was entirely free from cracks, and had only one ſmall ſcale running upon one ſide.

Theſe bars exhibited a tougher break, than thoſe drawn by hand; the colour was whiter, and the grain poſſeſſed a more regular and ſilky appearance.

(To be continued.)

⁂ *I have received a letter from Mr.* Boswell, *in which he expreſſes an opinion, that it is unfair in the Old Correſpondent whoſe letter appeared in our laſt Number to have applied Mr. B's apology to one particular part of his paper; as he conceives, that it ought to have been conſidered as indicating the ſpirit in which his whole communication was written. I have inſerted this ſhort notice out of reſpect to the writer; but have declined inſerting the letter itſelf, becauſe the controverſy can have no farther importance to the readers of this Journal, after the ſubject itſelf has been exhauſted.*



Mr. Churchman's
Topographical Plan

[illegible]

Fig 1

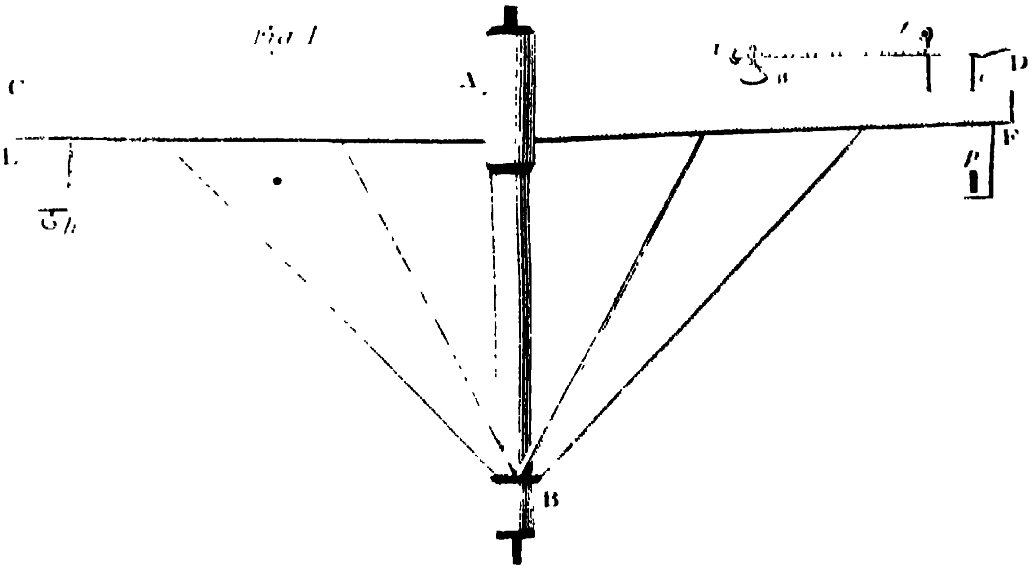


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Fig 2

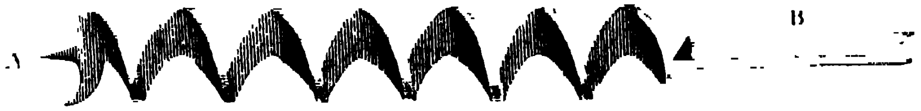

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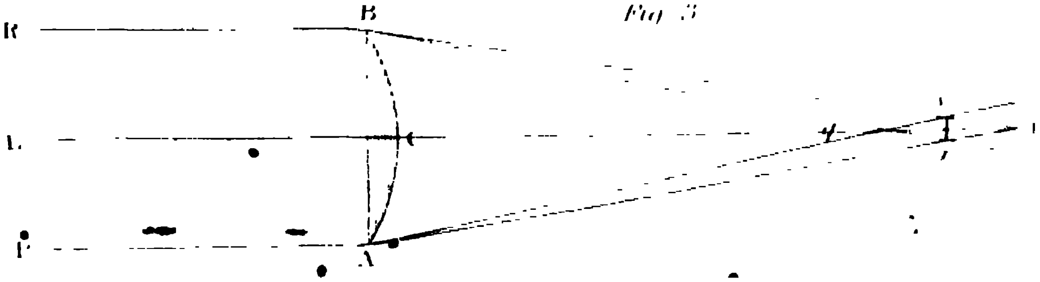
Fig 3



Mr. Seppings's Method of suspending Ships

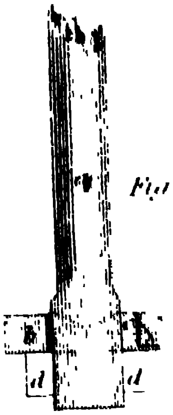


Fig. 3

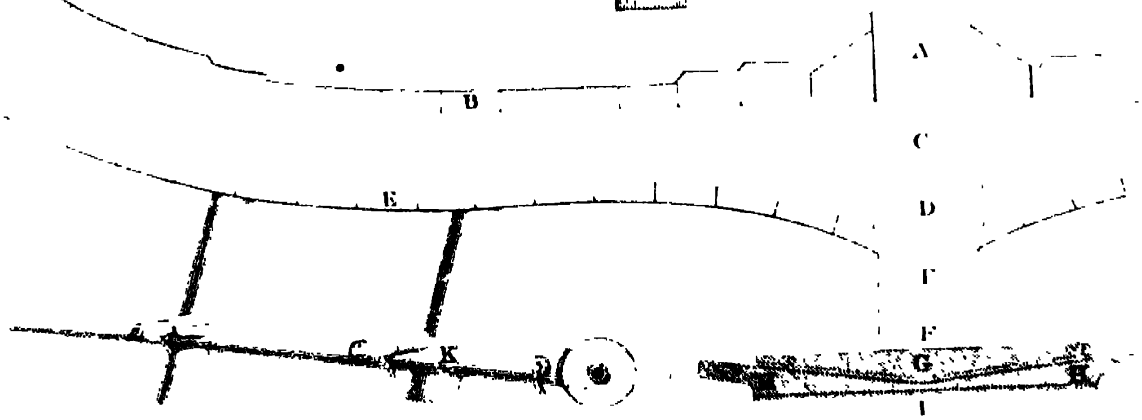
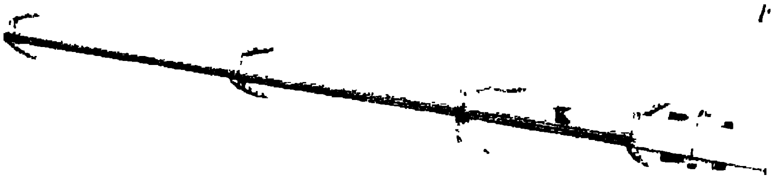


Fig. 2



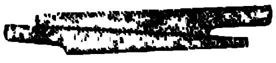
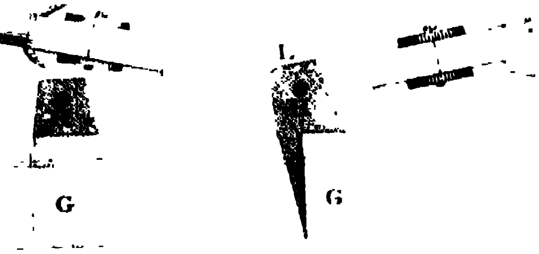
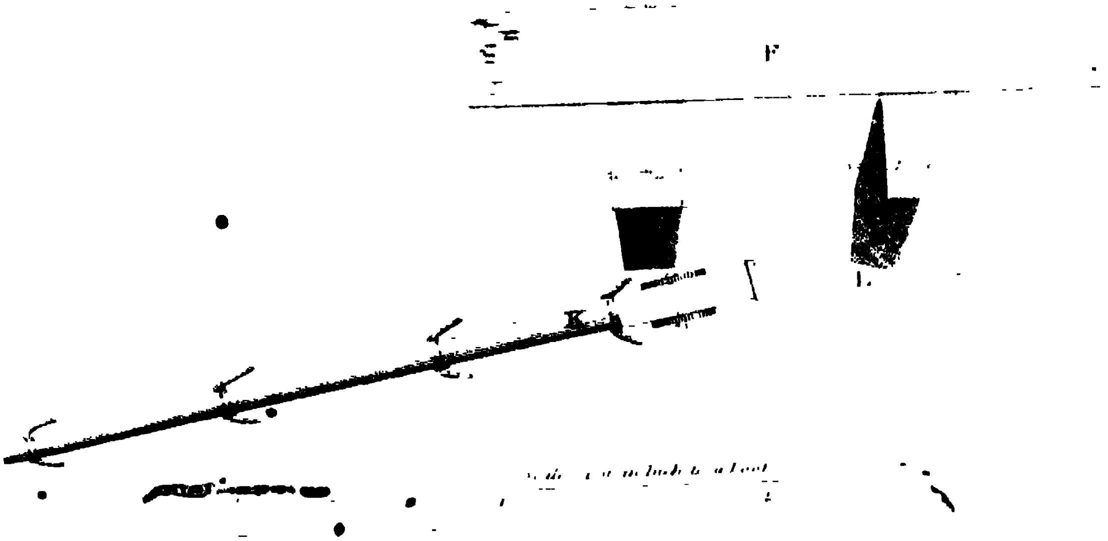


Fig. 1





Scale of an Inch to a Foot

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1805.

ARTICLE I.

An Account of ſome new Experiments which prove that the Temperature at which the Denſity of Water is a Maximum, is ſeveral Degrees of the Thermometer above the freezing Point. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c &c.*

Received July 16, 1805, *from the Author; with a Letter dated Munich,* 25*th June,* 1805.

Phenomena which have been explained from the maximum denſity of water being at higher than freezing temperature.

IN my ſeventh eſſay, in which I have treated of the propagation of heat in fluids, and alſo in a paper publiſhed in the Philoſophical Tranſactions for the Year 1804, Part I.; in which I have given an account of a curious phenomenon frequently obſerved on the Glaciers of Chamouny, I have aſcribed the melting of ice which is placed (by deſign, or by accident) below the ſurface of ice-cold water, to currents of warmer water, which, in certain caſes, are ſuppoſed to deſcend in hat ice-cold liquid: but as this ſuppoſed fact has lately been alled in queſtion by ſeveral perſons, and as the explanations have founded on it muſt fall to the ground, unleſs it can be upported, I have been induced to re-conſider the matter, nd to give it a careful and thorough inveſtigation.

The fact queſtioned

It was firſt announced by De Luc.

The fundamental fact on which the ſuppoſition is grounded, which was announced many years ago by Mr. De Luc; namely, that the temperature at which the denſity of water is a maximum, is conſiderably higher than that at which that fluid freezes, is indeed ſo very extraordinary, and appears to be the cauſe of ſo many intereſting phenomena, that too much pains cannot be taken to put it beyond doubt.

New method of proving it.

As the methods hitherto uſed for determining that important point have, by ſome at leaſt, been conſidered as inſufficient, I ſhall take the liberty to propoſe another, by which the fact in queſtion may, I think, be demonſtrated directly; and without any nice calculations, or any very difficult or delicate experiments.

Let the following experiments, (which it will be eaſy to repeat) ſpeak for themſelves.

Apparatus. In the middle of a thin cylindrical braſs veſſel, a thin braſs cup is ſupported.

Having provided a cylindrical veſſel, (A Fig. 1. Plate XII.) open above, made of thin ſheet braſs, which is 5½ inches in diameter, and four inches deep, ſupported on three ſtrong legs, 1¼ inches high; I placed in it a thin braſs cup, (B) two inches in diameter at its bottom, (which is a little convex downwards) 2 $\frac{8}{10}$ inches wide at its brim; and 1 $\frac{3}{10}$ inches deep; which cup ſtands on three ſpreading legs made of ſtrong braſs wire, and of ſuch form and length, that when the cup is introduced into the cylindrical veſſel, it remains firmly fixed in the axis of it, and in ſuch a ſituation, that the bottom of the cup is elevated juſt 1¼ inches above the bottom of the cylindrical veſſel.

In the middle of the braſs cup is ſupported

In the middle of this cup there ſtands a vertical tube of thin ſheet braſs, ½ an inch in diameter, and $\frac{6}{10}$ of an inch in length, open above, which ſerves as a ſupport for another ſmaller cup (C) which is made of cork; the brim of which is on the ſame horizontal level with the brim of the larger braſs cup, in which it is placed.

an hemiſpherical cup of cork,

This cork cup, which is ſpherical, (being ſomething leſs than half of an hollow ſphere) is one inch in diameter at its brim, meaſured within $\frac{4}{10}$ of an inch deep, and ¼ of an inch in thickneſs. It is firmly attached to the vertical tube on which it ſtands, by means of a cylindrical foot, ½ an inch in diameter, and ¼ of an inch high; which, when ſome force is employed, enters into the opening of the vertical tube.

On one ſide of this cork cup there is a ſmall opening, which receives, and in which is confined the lower extremity of the tube of a ſmall mercurial thermometer, (D). The bulb of this thermometer, which is ſpherical, is juſt $\frac{3}{10}$ of an inch in diameter, and it is ſo fixed in the middle of the cup, that its centre is juſt $\frac{1}{4}$ of an inch above the bottom of the cup; conſequently it does not touch the cup any where, nor does any part of it project above the level of its brim.

having in its center the bulb of

The tube of this thermometer, which is ſix inches in length, has an elbow near its lower end, at the diſtance of one inch from its bulb, which elbow forms an angle of about 110 degrees, and the thermometer is ſo fixed in the cup, that the ſhort branch of its tube, namely, that to the end of which the bulb is attached, lies in an horizontal poſition, while the longer branch (to which a ſcale, made of ivory, and graduated according to Fahrenheit is affixed,) projects obliquely upwards and outwards, in ſuch a manner that the freezing point of the ſcale lies juſt above the level of the top of the cylindrical veſſel in which the cups are placed.

a thermometer.

The cork cup, which was turned in the lathe, is neatly formed, and in order to cloſe the pores of the cork, it was covered, within and without, with a thin coating of melted wax; which was poliſhed after the wax was cold.

Other particulars.

The thermometer was fixed to the cork cup by means of wax, and in doing this, care was taken to preſerve the regular form of the cup, both within and without.

The vertical braſs tube which ſupports this cup in the axis of the braſs cup, is pierced with many holes, in order to allow a free paſſage into it, and through its ſides, to the water employed in the experiments.

Having attached about ſix ounces of lead to each of the legs of the braſs cup, in order to render it the more ſteady in its place, it was now introduced with its contents, into the cylindrical veſſel, and the veſſel was placed in an earthen baſin (E) ſeven inches in diameter below, 11 inches in diameter at its brim, and five inches deep, and was ſurrounded on all ſides with pounded ice.

This apparatus was placed in a pan, and ſurrounded with ice.

Several flat cakes of ſolid ice were now put into the cylindrical veſſel, and faſtened down upon its bottom, and under the bottom of the braſs cup, and a circular row of other long pieces of ice were placed, in a vertical poſition, round the

Ice was placed and ſecured in the cylindrical veſſel, but not in the cups, and ice-cold water was poured into all the remaining ſpace.

outſide of the brim of the braſs cup, between it and the vertical ſide of the cylindrical veſſel, which vertical pieces of ice reached upwards to within about $\frac{1}{10}$ of an inch of the top or brim of the cylindrical veſſel; and when this was done, ice-cold water was poured into this veſſel till the ſurface of that cold fluid ſtood juſt one inch above the level of the brim of the cork cup.—Both cups were of courſe ſubmerged and filled with ice-cold water, and ſurrounded on all ſides by ſolid cakes of ice.

The cups remained thus ſubmerged for one hour.

After things had remained in this ſituation more than an hour, during which time the cold water in the cylindrical veſſel, and that in the cups, was frequently agitated with the ſoft end of a ſtrong feather, and the cups and the water in every part of the veſſel appearing to be exactly at the temperature of freezing: I proceeded to make the following deciſive experiment.

Experiment. No. 1.

Experiment I. A cone of metal at 42° was plunged in the ice-cold water, juſt above the cork cup:

A ſolid ball (F) of tin having been provided, two inches in diameter, with a cylindrical projection on the lower ſide of it, one inch in diameter, and half an inch long, ending in a conical point, which projected (downwards) half an inch farther; this ball (to which was fixed a ſtrong iron wire ſix inches in length, which ſerved as a handle to it,) having been made to acquire the temperature of 42° F. by keeping it immerſed near half an hour in a large quantity of water at that temperature, was placed as expeditiouſly as poſſible over the middle of the cork cup, and in ſuch a ſituation that the whole of the deſcending conical point (half an inch in length) was immerſed in the ice-cold water in the cylindrical veſſel, the extremity of that point being juſt half an inch perpendicular above the upper ſide of the bulb of the thermometer which lay in the cork cup.

It was foreſeen, that if the water in contact with the cone became denſer by the heat, it would deſcend and raiſe the thermometer.

I knew that the particles of ice-cold water which were thus brought into contact with the conical point, could not fail to acquire ſome ſmall degree of heat from that relatively warm metal; and I concluded, that if the particles of water ſo warmed ſhould in fact become *heavier* than they were before, in conſequence of this ſmall increaſe of temperature, they muſt neceſſarily *deſcend* in the ſurrounding lighter ice-cold liquid, and as the heated metallic point was placed directly over the cork cup; and fixed immoveably in that ſituation, I foreſaw that the deſcending current of warm water muſt neceſſarily fall into that cup, and at length fill it, and that the preſence of this warm water in the cup would be announced by the riſing of the thermometer.

It did, in fact, raiſe the thermometer in the cup;

The reſult of this very intereſting experiment was juſt what I expected: the conical metallic point had not been in contact with the ice-cold water more than 20 ſeconds, when the mercury in the thermometer began to riſe, and in three minutes it had riſen three degrees and a half, namely, from 32° to $35\frac{1}{2}$°, when five minutes had elapſed, it had riſen to 36°, when an end was put to the experiment.

but it did not affect a thermometer near the ſurface of the water;

Another ſmall thermometer, placed juſt below the ſurface of the ice-cold water, and only $\frac{2}{10}$ of an inch from the upper part of the conical point, on one ſide of it, did not appear to be ſenſibly affected by the vicinity of that warm body.

nor another near the outſide of the cup.

A third thermometer, the bulb of which was placed in the braſs cup, juſt on the outſide of the cork cup, and on a level with its brim, ſhewed that the water which immediately ſurrounded the cork cup, remained conſtantly at the temperature of freezing, during the whole time that the experiment laſted.

The metal at 42° could not the water to the ſame temp.

As I well knew, from the reſults of the experiments on the propagation of heat in a ſolid bar of metal, of which an account has been given in a memoir preſented to the firſt claſs of the National Inſtitute of France, on the 7th of May 1804, that the ice-cold water in this experiment could not poſſibly acquire from a contact with the conical metallic point, a temperature ſo high as that of 42°, I was by no means ſurpriſed to find that the thermometer belonging to the cork cup roſe no higher.

A ſomewhat greater heat propoſed to be applied.

In order to ſee if it could not be made to riſe not only higher, but alſo more rapidly, by employing the metallic ball heated to ſuch a temperature as it might by ſuppoſed would be ſufficient to heat thoſe particles of ice-cold water which ſhould come into contact with its conical point, to the temperature at which the denſity of water is ſuppoſed to be a maximum, I made the following experiments.

Experiment. No. 2.

Experiment 2. The former experiment was

Having removed the ball, I gently bruſhed away the warm water, which, in the laſt experiment had been lodged in the cavity repeated; the cone being heated to 60°.

cavity of the cork cup, (and which ſtill remained there, as was evident from the indication of the thermometer belonging to the cup,) I placed ſeveral ſmall cakes of ice in the cylindrical veſſel, which ice floating on the ſurface of the water in the veſſel, prevented that water from receiving heat from the air of the atmoſphere, which at that time was at the temperature of 76° F. And as the cork cup had been a little heated by the warm water in the foregoing experiment, time was now given it to cool.

As ſoon as the cup, and the whole maſs of the water in the cylindrical veſſel appeared to have acquired the temperature of freezing, I carefully removed the cakes of ice which floated on the ſurface of the water, and introduced once more the projecting conical point belonging to the metallic ball into the ice-cold water in the veſſel, placing it exactly in the ſame place which it had occupied in the foregoing experiment; but this ball, inſtead of being at the temperature of 42° F. as before, was now at the temperature of 60° F.

The effect was more rapid and more conſiderable.

The reſult of this experiment was very ſtriking, and if I am not much miſtaken, affords a direct, unexceptionable, and demonſtrative proof, not only that the maximum of the denſity of water is in fact at a temperature which is ſeveral degrees above the point of freezing; but alſo, that warm currents do actually ſet downwards in ice-cold water, whenever a certain ſmall degree of heat is applied to the particles of that fluid which are at its ſurface.

Particulars.

The conical metallic point had been in its place no more than ten ſeconds when I diſtinctly ſaw that the mercury in the thermometer belonging to the cork cup was in motion; and, when 50 ſeconds had elapſed, it has riſen four degrees, viz. from 32° to 36°.

When two minutes and a half had elapſed, (reckoning from the moment when the metallic point was introduced into the cold water,) the thermometer had riſen to 39°, and at the end of ſix minutes to 39⅞°, when it began to fall; but very ſlowly however, for at the end of eight minutes and a half it was at 39¾°.

A thermometer near the outſide of the cup was not affected.

A ſmall mercurial thermometer, the bulb of which was placed on one ſide of the cork cup, at the diſtance of about $\frac{2}{10}$ of an inch from it, ſhewed no ſigns of being in the leaſt affected by the heat communicated to the ice-cold water by the metallic ball.

This experiment was repeated three times the ſame day, (the 13th of June 1805,) and always with nearly the ſame reſults.

The mean reſults of theſe four experiments were as follows:

Tabulated reſults of the experiments repeated, with the cone at a low heat.

Time elapſed reckoned from the beginning of the experiment.	Min.	Sec.		Temperature of the water in the cork cup, as ſhewn by the thermometer.
	0	0		32°
At	0	10	began to riſe	32+
At	0	23	had riſen to	33
	0	28		34
	0	35		35
	0	48		36
	1	3		37
	1	35		38
	2	32		39
	3	41		39½
	4	48		39¾
	6	5		39⅞

As I had found by ſome of my experiments made in the year 1797, of which an account is given in my ſeventh eſſay, part I. that water at the temperature of about 42° F. and conſequently what we ſhould call very cold, melted conſiderably more ice, when ſtanding on it, than an equal quantity of boiling hot water, in the ſame ſituation, I was very curious to ſee whether the thermometer, the bulb of which lay in the cork cup, would not alſo be leſs heated by the ball when it ſhould be applied *very hot*, to the ſurface of the water, than when its temperature was much lower.

Whether a much greater heat in the metal would affect the water in the cup leſs.

To determine that point I made the following experiment.

Experiment. No. 3.

The cylindrical veſſel with its contents having been once more reduced to the uniform temperature of freezing water, the metallic ball was heated in boiling water, and being as expeditiouſly as poſſible taken out of that hot liquid, its projecting conical point was ſuddenly ſubmerged in the ice-cold water, as in the former experiments.

Experiment 3. The experiment repeated with the cone heated to 212°.

The reſult of this experiment was very intereſting, and it ſeems to me to throw much light on the ſubject of theſe inveſtigations.

Effect on the thermometer in the cup much leſs.

It was not till 50 ſeconds had elapſed that the thermometer began to ſhew any ſigns of riſing, and at the end of one minute and ſeven ſeconds it had only riſen two degrees.

In the foregoing experiments, when the metallic ball was ſo much colder, the thermometer began to riſe in ten ſeconds, and at the end of one minute and three ſeconds it had riſen five degrees.

This difference is very remarkable; and if it does not prove the exiſtence, and great efficacy of currents, in conveying heat in fluids, I muſt confeſs that I do not ſee how the exiſtence of any inviſible mechanical operation, the progreſs of which does not immediately fall under the cognizance of our ſenſes, can ever be demonſtrated

As the experiment made with the ball heated in boiling water appeared to me to be very intereſting, I repeated it twice, and its reſults were always nearly the ſame.

Tabulated reſults of experiments repeated; with the cone at a conſiderable heat.

The mean reſults of theſe three experiments were as follows:

Time elapſed, reckoned from the beginning of the experiment.				Temperature of the water in the cork cup, as ſhewn by the thermometer.
	Min.	Sec.		
	0	0		32°
At	0	50	the thermometer begun to riſe.	32+
At	1	2	had riſen to	33
	1	7		34
	1	18		35
	2	2		36
	3	2		36½
	4	17		37
	6	12		38
	7	17		38⅛
	9	0		38¼
	12	0		38¼
	14	0		38¼

Comparative view of the experiments.

By comparing the mean reſults of theſe experiments with the mean reſults of thoſe in which the ball was at the temperature of 60°, we may ſee how much more rapidly the thermometer in the cork cup acquired heat when the metallic ball was *relatively cold* than when it was at the temperature of boiling water; and it is more than probable that it was not till after the conical metallic point had been conſiderably cooled, by a contact with the ice-cold water, that thoſe ſtreams of moderately warmed water began to deſcend from it, by which the thermometer was at length heated.

In the experiments made with the ball heated in boiling water, a ſmall thermometer, placed juſt under the ſurface of the ice-cold water, on one ſide of the metallic point, began to riſe very rapidly as ſoon as the hot ball was fixed in its place; but another thermometer placed about half an inch lower, on one ſide of the cork cup, remained to all appearance at perfect reſt, from the beginning of the experiment to the end of it.

When the very hot cone was uſed, a thermometer at the ſurface was heated;

—but a lower one was not.

The explanation of all theſe appearances is ſo eaſy that it would be a waſte of time to ſay much on that ſubject. It may however be uſeful to recapitulate the principal phenomena, and ſhew in what manner they tend to eſtabliſh the facts which they are brought to prove.

Explanation:

Every body muſt ſee, at the firſt glance, that in all theſe experiments the heat which cauſed the thermometer to riſe was *carried* down into the cork cup by deſcending currents of warm water; and it is evident that water which deſcends muſt of neceſſity be ſpecifically heavier than that in which it deſcends.

That the ſlightly heated water deſcends;

From the reſults of theſe experiments we may conclude that the denſity of water is a maximum when that fluid is at a temperature ſomewhat lower than that of the *fortieth degree* of Fahrenheit's thermometer.

—and the greateſt denſity is rather below 40°.

In all the foregoing experiments, more or leſs warm water deſcended through the ice-cold water into the cork cup; but the reſults of the experiments which were made with the metallic ball heated in boiling water, ſhew evidently that when the particles of water at or near the ſurface of a quieſcent maſs of ice-cold water, are by any means heated to a temperature ſeveral degrees above that of 40° of the ſcale of Fahrenheit, ſuch particles, ſo heated, become ſpecifically lighter than ice-cold water, and conſequently cannot deſcend in that cold and denſer liquid.

Hotter water is lighter and aſcends.

With the hot cone ſome of the fluid aſcended and ſome deſcended.

In the experiments in queſtion, (with the hot ball) ſome of the particles of the water, namely, thoſe which came firſt into contact with the conical metallic point, were heated to a higher temperature than that at which they were diſpoſed to ſink in ice-cold water; and theſe roſe and ſpread themſelves over the ſurface of the cold liquid; but others, which happened to acquire leſs heat, deſcended in it, and after filling the cork cup, overflowed, no doubt its brim, and then deſcending to the bottom of the braſs cup, and coming into contact with that ice-cold metal, were there cooled, and there remained at reſt.

Cork a good non-conductor.

As cork is an excellent non-conductor of heat, the warm water accumulated in the cork cup during an experiment retained its heat a long time after the heated ball was removed, notwithſtanding its being ſurrounded on all ſides, and even covered immediately by ice-cold water; (which by the by is a pretty ſtrong proof that water is by no means a good conductor of heat) care however was taken, not only to remove this warm water after each experiment, by bruſhing it away with the ſoft end of a ſtrong feather or quill, but alſo to cool the cup, and reduce it to the temperature of freezing water; which laſt operation was found to be much accelerated by bruſhing it out frequently with the feather. In order that this feather might itſelf be ice-cold, it was ſuffered to remain conſtantly in the ice-cold water, in the cylindrical veſſel.

Cautions.

Apparatus by which the cone was fixed.

I muſt not forget to give an account of the means uſed for fixing the metallic ball in its place. This was done in a very ſimple and effectual manner. A ſlip of ſtrong tin (G H) ſix inches long, and 2¼ inches wide, with a circular hole in the middle of it, one inch in diameter, being laid horizontally on the top or brim of the cylindrical veſſel, in ſuch a manner that the center of that circular hole coincided with the axis of the cylindrical veſſel, the ſhort cylindrical projection belonging to the ball being introduced into that hole, the ball was firmly ſupported in its proper place.

The quantity of ice-cold water in the cylindrical veſſel was ſo regulated that the whole of the conical point being ſubmerged, the ſurface of the water was on a level with the lower end of the cylinder, or, which is the ſame thing, with the baſe of the inverted cone.

When not only the whole of the conical point, but a part alſo of the ſhort cylinder were immerſed in the ice-cold water, the warmed water appeared to be thrown into eddies in its deſcent, which diſperſing about prevented its falling regularly in one continued ſtream into the cork cup.

The conical figure preferable for the immerſed metal.

To *conclude*, I would juſt obſerve, that although the foregoing experiments appear to me to be perfectly unexceptionable, and that their reſults afford demonſtrative proof of the facts which they were contrived to eſtabliſh; yet, when attempts are made by experiments ſimilar to theſe, to determine whether heat can be made to paſs *downwards* in water which is at a higher temperature than that at which its denſity is a maximum, difficulties occur which appear to me to be quite inſurmountable.

It is not eaſy to make concluſive experiments at elevated temperatures:

The fluidity of water is ſo perfect, or the mobility of its particles ſo great, that the liquid at the ſurface, which is firſt heated and rarefied, immediately ſpreads far and wide, and meeting with the ſides of the containing veſſel, heats them, and this heat, ſo acquired, making its way downwards (as well as upwards) in the ſolid ſubſtance of which the veſſel is conſtructed, raiſes the temperature of the lower ſtrata of the fluid in contact with it, which moving towards the axis of the veſſel, communicates heat to a thermometer placed there, below the ſurface of the water.

For the ſides of the veſſel greatly modify the effects by conducting the heat, &c.

That theſe various operations do in fact take place, nobody can doubt: and it appears to me to be the more probable that all the heat which a thermometer placed below the ſurface of warm water acquires when a great degree of heat is applied to the particles of that fluid which are at the ſurface, is in fact received from the ſides of the containing veſſel, not only becauſe the thermometer acquires heat ſo very ſlowly, but alſo, and more eſpecially, becauſe this heat is acquired much more ſlowly when the containing veſſel is wide than when it is narrow; and alſo when it is made of a ſubſtance which is a good conductor of heat than when it is conſtructed of a ſubſtance which is a bad conductor of heat, as I have found by experiment. But as this particular enquiry is foreign to my preſent purpoſe, I ſhall not enlarge on it in this place.

Facts which ſhew the probability that conſiderable heats are chiefly tranſmitted downwards through fluids by the agency of the veſſel.

II.

Deſcription of a Preſs for preſerving botanical Subjects; with an Account of the Succeſs of the Improvement in the Art of Blaſting pointed out by Mr. Jessop *in this Journal. In a Letter from Mr.* Tho. Harrison.

To Mr. NICHOLSON.

SIR,

Introductory obſ. on preſerving plants.

BEING deſirous to make a collection of the plants which grow in the neighbourhood of Kendal, and to preſerve them in an herbarium, I was naturally led to a conſideration of the different methods, which have been uſed by botaniſts, of drying and preſerving ſpecimens; but finding none ſo complete and expeditious as I could wiſh, I therefore adopted a plan of my own, which I am perſuaded will be found to anſwer every purpoſe that the botaniſt can require. The real utility which is to be derived by a young botaniſt from the preſervation of plants, is the power of future examination, and to anſwer the purpoſe the natural appearance of each ſpecimen ought to be preſerved as calculated to produce this effect; for ſince the ſeed-veſſels and ſtems of plants occupy much more ſpace in thickneſs than the leaves, in order to preſerve the latter from ſhrivelling, the two former muſt generally be bruiſed in ſuch a manner as totally to preclude any accurate inveſtigation afterwards: and this on trial is found to be the caſe: beſides, all the plants that happen to be in the preſs at the ſame time, however various their texture, are ſubjected to the ſame degree of preſſure.

Remarks on the methods now in uſe.

The plan recommended by T. Velley, Eſq. ſeems equally exceptionable; I mean the method of placing the plant when freſh between ſeveral ſheets of blotting-paper, and ironing it with a large ſmooth heater pretty ſtrongly warmed, till all the moiſture is diſſipated. By this method it is evident that the parts of fructification muſt be much bruiſed: it may preſerve the colour of the bloſſoms better than any other, but this in the ſcience of botany is not very eſſential, at leaſt if it was, I believe that no method hitherto diſcovered will ſucceed univerſally in arreſting the fading beauty of delicate flowers. The next plan which is given by Dr. Withering, is one uſed by a Mr. Whateley a ſurgeon, but his contrivance, though certainly better adapted to preſerve the parts of fructification and the ſhape of the ſtem entire than the two former, I found would take up much more time than a country ſurgeon can well ſpare; therefore this led me to make what I thought an improvement on Mr. Whateley's plan, and the experienced botaniſt is left to judge of my ſucceſs from the following deſcription; this I can ſay for it, that it anſwers completely (of courſe) to my own wiſh; but further, that Mr. John Gough, of Middleſhaw, who has the beſt collection of plants in this county, regrets that he had not the ſame contrivance, and has urged me much to ſend you the following deſcription of my preſs.

The author's new preſs deſcribed. It conſiſts of a ſeries of trays or boxes, having (all but the loweſt) canvas bottoms. They contain ſand, and the plants are laid between box and box.

This inſtrument conſiſts of 17 oblong boxes, each, excepting the uppermoſt and lowermoſt, is made of four ſides of well-ſeaſoned oak wood, two inches deep and one-fourth of an inch thick, dove-tailed together; the two end-pieces of which have two notches each in the middle wherein to place the fingers in lifting it; and the bottom conſiſts of canvas glued and nailed to the wooden frame: in each corner is fixed a ſmall triangular piece of wood reaching half an inch from the bottom upwards (for a purpoſe hereafter to be explained). Theſe boxes are made ſo as to be placed one within another ſucceſſively upwards, the loweſt (I am ſpeaking only of the 15 with canvas bottoms) meaſures 20 inches by 16 on the outſide, and the higheſt 12 inches by 8. The bottomeſt, or carriage on which all the others are ſupported, is much ſtronger, the ſides of it being two inches deep and three quarters of an inch thick: its bottom is of wood of the ſame thickneſs, all over perforated with holes one-third of an inch in diameter, and reſts on four iron caſtors, one at each corner, to render the whole more eaſily moveable; at each end of it there is an iron handle: This box is 20 inches by 16 within, and will therefore barely receive the largeſt of the boxes with canvas bottoms. The uppermoſt, or 17th box, is of the ſame conſtruction as the canvas boxes, excepting that its bottom is of wood of the ſame thickneſs as its ſides, with a number of holes pierced through of the ſame magnitude as thoſe in the carriage, and canvas glued to the under ſide of it. To complete this preſs, two folds of blotting-paper are to be placed over the holes in the bottom of the carriage, in which, as well as all the canvas-boxes, muſt be put an inch in depth of the fineſt ſand, waſhed and ſifted; river ſand is moſt eligible, as being leaſt angular; which ſand muſt be made level on the top by drawing over it a rule that reaches one inch into each box; theſe boxes are placed one within another, and each by containing an inch depth of ſand, raiſes the incumbent frame one inch, ſo that when they are thus charged and placed one upon another, the whole makes a truncated pyramid of 18 inches in height, and weighing about 1 cwt. The uppermoſt box may be filled with ſand, or, if more weight be neceſſary, with ſhot. To this box I have added (what is by no means eſſential) a cap, conſiſting of the ſides of three boxes fixed one within another, each projecting one inch above its inferior, for the ſake of uniformity; the top is covered by a thin board: when this cap is placed upon the uppermoſt box, the whole has not an inelegant appearance.

Reference to the engraving.

Leſt the deſcription ſhould not be ſufficiently clear, I have ſketched the following figure, repreſenting a perpendicular ſection of the preſs viewed on one ſide of it. *Plate XV.*

Fig. 1. repreſents a perpendicular ſection of the preſs made lengthwiſe.

a. The end of the bottomeſt box or carriage.

b. Its bottom part perforated with holes.

c. The end of the firſt box with a canvas bottom.

d. The canvas bottom.

e. The ſand which but half fills the box.

f. The 17th or uppermoſt box, with a perforated wooden bottom.

g g g. Repreſent the end of the cap formed of the frames of three boxes, in the empty ſpace of which may be placed the botaniſt's pocket box.

Fig. 2. repreſents the rule wherewith the ſand is made level in the boxes; it is 18 inches long.

h. A notch one inch deep.

i. The edge made thin.

k. A notch an inch long.

Fig. 3. repreſents the end of a canvas box.

l l. The two notches to place the fingers in when the box is to be lifted.

The

The Method of uſing it.

Method of uſing it.

The plants are laid between blotting-paper, and the preſſure regulated by the number of boxes above each, &c.

After a ſpecimen is ſelected, a ſlip of paper is to be attached to it, containing its name, place of growth, and time of gathering. The boxes are then to be taken off by three or four at a time, until the number removed may be judged to be of ſufficient weight to produce the neceſſary preſſure. On the ſand in the box thus ſelected for uſe, a ſingle fold of blotting-paper muſt be placed; upon that the plant, taking care to preſerve as much as poſſible the natural poſition of its characteriſtic parts: Over the plant a ſecond fold of paper muſt be laid, and then the boxes are to be replaced. If the plant be woody and require much preſſure, place it near the bottom; but if ſucculent, near the top; and herein conſiſts one great advantage of this preſs over every other, ſince the preſſure can be ſelected or varied at pleaſure, and all the parts of the plant preſſed equally; for if the plant be ſucculent, the preſſure can ſcarcely be too ſlight at firſt, and may be gradually increaſed without ever bruiſing the tendereſt part; and if, on the other hand, ſtrong preſſure be required, the plant may be placed in the bottomeſt box; and if the weight of ſand be not thought ſufficient, any number of the boxes next above it may be loaden with ſmall ſhot, by which the preſſure may be increaſed until the boxes be full, when the preſſure exerted would be much greater than the ſtrongeſt plants could bear without having ſome of its eſſential parts defaced. Another advantage which led me to give a preference to this method of preſſing plants, was the great ſaving of time; for though my profeſſional avocations ſhould require my immediate preſence at a diſtance, and thus drag me from this innocent amuſement when in the buſieſt part of claſſing and arranging my ſpecimens, and though my preſs ſhould be completely ſeparated into its conſtituent parts, yet the whole can be arranged, in the ſpace of one minute, to remain until the next leiſure time that may occur; ſo that, in fact, with this contrivance, botany is a ſtudy that can be entered into or given up as it may ſuit the convenience of a moment; which is not the caſe where any of the plans deſcribed in Dr. Withering's Introduction to the Study of Botany * are followed.

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* Third Edition.

Remarks on the effects and advantages of this contrivance.

It may be neceſſary to explain ſome parts of the ſtructure of the preſs. The holes in the bottomeſt and uppermoſt boxes are to admit of the circulation of air and the evaporation of moiſture, which will be conſiderable when the preſs without its cap is placed within the influence of the ſun or fire, as it always ought to be: and the ſmall triangular pieces of wood in the corners of each box, are to prevent the upper boxes from preſſing ſolely on the canvas bottom of the loweſt box, when any number are lifted together: theſe may at firſt ſight appear likely to impede the preſſure of the ſand when placed in the preſs, but this objection will vaniſh when it is recollected that each box contains an inch depth of ſand: and it is evident, on examination of the preſs, that the ſides of the boxes ſerve no other purpoſe than to keep the ſand together; for they are perceptibly moveable in a perpendicular direction, and ſuſtain no preſſure from the ſuperior boxes; therefore the plant is placed as if in a heap of ſand.

The beſt method of preſerving dried plants is to ſtitch them to a paper.

I am afraid I have already treſpaſſed too long on the attention of the reader, but I cannot diſmiſs the ſubject without offering a few obſervations on the manner of preſerving plants after they are dried. There have been at leaſt three methods uſed: firſt, fixing them to paper by the aid of gum-water: ſecond, placing them looſely in a book: third, ſtitching them with thread to a ſheet of paper. The firſt plan appears to me the leaſt eligible, for it cannot be neatly executed unleſs the plant be preſſed nearly flat, and by fixing it to the paper it can never admit of an accurate examination in future, but becomes a mere picture, and a very imperfect one; for almoſt every diſtinguiſhing character, eſpecially the parts of fructification, are injured or defaced; even the general habit is often ſo much altered that the plant could not be aſcertained, were it not for the name generally ſubſcribed at its root. The ſecond plan is alſo objectionable; for though the plant may at any time be ſubjected to examination, yet by lying looſe in a book, it is very apt to be broken by the leaſt motion, particularly when kept dry, which all preſerved plants ought to be. The third appears to me the leaſt liable to objection; for if the plant be neatly ſtitched down, it will not be ſubject to ſuch motion as to injure its parts; and by cutting the threads it may be detached from the paper, and ſubjected to any future inveſtigation.

I am, Sir,
Your's, &c.
Kendal, July 8, 1805. THOMAS HARRISON.

Account of the ſucceſsful application of the method of blaſting rocks indicated in this Journal by W. Jeſſop, Eſq.

P. S. I cannot refrain from calling to the recollection of your readers a valuable paper in the 9th Vol. p. 230, of your Journal, recommending the uſe of ſand in the blaſting of rocks. The effects there related of this ſimple agent, I confeſs did aſtoniſh me more than any thing I ever read: and as very ſerious and even fatal accidents have happened by the method of ramming blaſts with ſtone in the limeſtone and ſlate quarries in this neighbourhood, I was determined to repeat the experiment the firſt opportunity, and thus ſhew its effects to the men who work in thoſe places. Accordingly no long period elapſed before I had occaſion to be in the neighbourhood of the ſlate quarries in Longſliddale, about ten miles from Kendal; when I went to one of thoſe quarries, taking with me a bag of ſmall ſand, conſiſting of powdered freeſtone uſed in this country to ſcour pans with, and ſtrew upon the ſtone floors. I mentioned my buſineſs to one of the workmen who was then engaged in boring a hole 24 inches deep and about an inch diameter in a ſlate rock; the ſtratum was about 27 inches thick, and reclined from the perpendicular about 20 degrees; and this being the firſt blaſt, it was conſequently firmly ſurrounded and fixed in on all ſides by ſolid rock: The part that he wanted to throw out by the blaſt was ſuppoſed to be five ton weight: The direction of the hole which he was boring was perpendicular to the ſtratum, and therefore elevated above the horizon about 20 degrees. He ſmiled at my propoſal, but ſaid he would try the experiment to ſatisfy me, provided I would pay for the powder if it failed; but he ſeemed to think that I had a very poor idea of blaſting, to believe that a little light ſand would anſwer the purpoſe of the laborious and dangerous proceſs of ramming which he had been accuſtomed to uſe. However, he charged his blaſt with powder in the manner he had been uſed to do; *i. e.* into this hole

Safe and eaſy method of blaſting rock..

of 24 inches in depth, he put as much powder, together with two or three pieces of broken ſticks, of half an inch diameter and about four or five inches long, as reached 12 inches upwards. The ſticks he introduced for the purpoſe of keeping the powder looſe in the hole, ſo that it might take fire more nearly all at the ſame inſtant: This done, he put in a charged ſtraw, into the outer end of which was fixed a match-paper; and laſtly, he (with a ſmirking countenance, anticipating the joy he expected to feel at my diſappointment) filled up the hole with ſand. The match was then fired, upon which we removed to a diſtance, and the powder ſoon exploded; when he immediately exclaimed, from the peculiarity of the report, though the effect was unſeen by him, that the rock was as completely ſhattered as if the hole had been rammed with ſtone; and a view of the place verified the prediction. Since that time the plan has been adopted in that and moſt of the neighbouring quarries, with the uſe of ſmall ſand which is waſhed out of the river in floods, and the method anſwers as well as the old one which was attended with ſo much danger. In one caſe the ſand was thrown out twice without any effect on the rock; the third time, the hole was rammed in the uſual manner with ſtone, which was thrown out likewiſe; ſo that the new proceſs fortunately loſt no reputation.

I have detailed thus much in confirmation of the valuable paper before mentioned, and in hopes that it may induce ſome of your readers who reſide in the neighbourhood of mines or quarries, to endeavour to introduce this ſafe and eaſy method in lieu of the dangerous and tedious one of ramming with ſtone, in which proceſs we have had repeated inſtances of the loſs of limb, viſion, and even life itſelf: In the uſe of ſand there is comparatively no danger, and by adopting this plan many a valuable life will be ſaved.

III.

Deſcription of a Safety Valve, containing a Vacuum Valve in the ſame Hole of the Boiler. By Sir A. N. Edelcrantz.*

Large boilers require a ſafety valve and a vacuum valve.

IN large boilers or coppers, where boiling fluids are encloſed, a *ſafety valve* is generally uſed to prevent their burſting, from an unexpected exceſſive force of the elaſtic ſteam, and, beſides, a *vacuum valve*, to prevent their being compreſſed or cruſhed by the weight of external air, in the caſe of a ſudden condenſation of the vapours. Theſe two valves are commonly fitted in two different holes in the boiler; but as a more ſimple, and conſequently more eligible, method ſeems to be that of joining them together, I take the liberty to ſubmit to the Society for the Encouragement of Arts, &c. the following contrivance for that purpoſe:—

Deſcription of a valve that performs both offices.

a b, *Plate XII. Fig.* 2. is a common conical ſafety valve, fixed in the boiler *c d*, having four openings, *i i*, which are repreſented in a plan view in *Fig* 3: *e f* is the metallic rod, bearing the weight K K, with which the ſafety valve is loaded, and extending itſelf under that valve to *f*: *g h* is the *vacuum valve* conſiſting in a plane circular plate, with a braſs tube ſliding along the rod, and preſſed by a ſpiral ſpring to the ſafety valve *a b* (againſt which it has been well ground in making it), cloſing in that ſituation the openings *i i*.

It is a loaded valve opening upwards; having in its place a ſpring-valve opening downwards.

Such being the conſtruction of the whole, it is evident, that when the elaſticity of the ſteam increaſes, the two valves, joined together, with the holes *i i* ſhut, make but one, oppoſing to the elaſticity of the ſteam an united reſiſtance, which is regulated by the weight *k k*, in the common way; but, on the contrary, when by condenſation of the vapours a vacuum is produced, the external air in preſſing through *i i*, upon the vacuum valve *g h*, forces it down, and opens to itſelf a paſſage into the boiler.

The valve *g h* may eaſily be made conical, like the other, if that form ſhould be preferred; but in different trials, I have found planes, if well turned and ground together, join as perfectly as can be deſired, being preſſed by the united elaſticity of the ſpring and the ſteam.

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* From the Memoirs of the Society of Arts for 1804. The ſilver medal was awarded for this invention.

The ſame applied to a former contrivance.

Fig. 4. is the ſame contrivance adapted to a new kind of *ſafety valve* or *piſton*, which, though I originally intended it for the uſe of Papin's digeſters of a new conſtruction *, has been, in a larger ſize, applied by me even to ſteam engines, and is deſcribed in the Philoſophical Magazine of December, 1803.†

Other experiments.

I have lately begun, and ſhall purſue, a ſet of experiments, with the intention of regulating by this ſafety piſton, the quantity of admitted air to fire-grates, and to effect, by that means, a new mode of regulating the fire, and the elaſticity of ſteam in boilers, with leſs expenditure of fuel and of force than uſual; of which idea a hint is given in the work and place above mentioned. The reſult of theſe reſearches I ſhall at ſome future period do myſelf the honour of communicating to the Society.

IV.

Obſervations and Experiments for the Purpoſe of aſcertaining the definite Characters of the primary Animal Fluids, and to indicate their preſence by accurate chemical Teſts. By JOHN BOSTOCK, *M. D. Communicated by the Author.*

Great want of preciſion in the animal analyſis; particularly of the fluids.

THE preciſion which the analyſis of mineral and vegetable ſubſtances has attained, does not appear to be yet extended to the products of the animal kingdom. This remark may be applied both to the ſolids and fluids which compoſe the animal body, but it is the moſt applicable to the latter claſs of ſubſtances. The terms ſerous, mucilaginous, gelatinous, &c. are employed, even by the moſt eſteemed medical and phyſiological writers *, in a vague and indeterminate manner, without attending either to the original import of the word, or to the reſtricted meaning, which it is neceſſary to impoſe upon popular expreſſions when they are adopted in ſcientific reſearches. The object of the preſent paper is to aſcertain a definite character for what I propoſe to call the primary animal fluids, and to diſcover delicate and accurate teſts, by which their preſence may be eaſily and certainly indicated. By primary animal fluids, † I mean thoſe into which the compound fluids exiſting in the animal body are capable of being reſolved by the application of different re-agents, without decompoſing them into their ultimate elements.

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* Nicholſon's Journal, March, 1804.

† The deſcription of this contrivance being already publiſhed, it would be ſuperfluous to repeat it. I only beg leave to add the following practical remark. A metallic piſton, if well turned and fitted into a cylinder of exactly the ſame kind of metal, will probably have the ſame degree of expanſion, eſpecially if hollow, and conſequently will not increaſe its friction in any increaſed degree of temperature. But as in practice the cylinder is commonly expoſed to a lower temperature than the piſton, heated by the ſteam, a little increaſe of friction will take place by an increaſe of heat. To prevent the effect of this, I have found it uſeful to employ for the piſton a metal of ſomewhat leſs expanſive power than the cylinder: and the expanſion of red copper being to that of braſs nearly as 10 to 11, I prefer making the piſton of the former metal, when the cylinder is made of braſs.

Primary animal fluids.

Albumen.

Albumen, large component part of animals.

The firſt of the animal fluids which I propoſe to make the ſubject of my inveſtigation is the albumen. With the exception of water, no fluid appears to enter ſo largely into the compoſition of the animal body. It forms a very conſiderable proportion of the blood, and is found in greater or leſs quantity in nearly all the ſecretions. It is alſo capable of aſſuming the ſolid form, without undergoing any change in its chemical properties; in this ſtate it conſtitutes the baſis of all the membranous ſubſtances, which are ſo extenſively diſperſed through every part of the ſyſtem; it compoſes the cellular tiſſue into which the earth of the bones, and the fibrous matter of the muſcles are depoſited, while it enters largely into the ſtructure of the ſkin, the glands and the veſſels. At preſent, however, we ſhall direct our attention to it while in the liquid form.

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* Even Mr. Abernethy, in his late valuable work on tumors, ſpeaks of the gelatinous part of the blood, where I conceive from the context, there can be no doubt that he intended to deſignate the fibrine.

† All the animal fluids, both primary and compound, are merely ſolutions of a ſolid body in water; but thoſe ſubſtances which are moſt frequently ſeen in a ſtate of ſolution, have very generally obtained the title of fluids.

In

Moſt conveniently obtained from white of egg.

In order to obtain it in a ſtate of purity, I had recourſe to the white of the egg, a ſubſtance to which the name of albumen was originally applied, and which is ſtill conſidered by the moſt eminent chemiſts * as compoſed entirely of this ſubſtance. In order to aſcertain how far this opinion was correct, I kept a quantity of the white of the egg in a temperature of 212°, until it was firmly coagulated. It was then cut into ſmall pieces and placed in the upper part of a narrow-necked funnel, when a few drops of a browniſh viſcid fluid were ſeparated from it. Other pieces of the ſame coagulum were kept for ſome time in boiling water, the fluid being then paſſed through a filtre, had acquired a light browniſh colour, and a faint odour; when agitated it was ſlightly mucilaginous. By ſlow evaporation, a ſmall quantity of a brittle, ſemi-tranſparent ſubſtance was obtained. It appeared therefore evident, that the white of the egg contains a ſmall quantity of a ſubſtance incapable of coagulation, and therefore eſſentially different from albumen. I ſtill, however, continued to employ it for the purpoſe of aſcertaining the properties of albumen, as it affords this ſubſtance in a ſtate of greater purity than it can be obtained from any other ſource.

White of egg is not pure albumen; it contains a little of matter not coagulable.

White of egg contains 80 parts water; 15,5 ſolid albumen and 4,5 uncoagulable matter.

It was an *object* of ſome importance to aſcertain the proportion which this foreign ingredient bore to the albumen itſelf; 100 grains of firmly coagulated albumen were kept for ſome time in boiling water, and this being poured off, a freſh quantity was added, and this proceſs repeated until the water appeared to contain no farther impregnation. The whole of the fluid was then evaporated, and a reſiduum was left which amounted to 4½ grains. Beſides the admixture of this peculiar ſubſtance, the white of the egg contains a conſiderable quantity of water, not only in its liquid ſtate, but after it is coagulated. By a gentle heat the water may be evaporated, and the ſolid matter is left behind in the form of a hard, brittle, tranſparent ſubſtance; I have found that upon an average, ⅘ of recently coagulated albumen may be conſidered as conſiſting of water. Hence it will appear, that 100 grains of the white of egg conſiſt of 80 grains water, 4.5 grains of uncoagulable matter, and 15.5 grains only of pure albumen.

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* Hatchett, Phil. Tranſ. 1800, p. 375.
Thomſon's Chemiſtry, IV. p. 484.

The moſt diſtinguiſhing characteriſtic of albumen is the property of being coagulated by heat, which forms an obvious and eaſy teſt of its preſence, when it exiſts in a compound animal fluid in any conſiderable proportion. In order to aſcertain how ſmall a quantity of albumen could by this means be rendered viſible, I added 13 grains of the white of the egg to 87 grains of water, and thus formed a ſolution, one grain of which contained $\frac{1}{50}$ grain of pure albumen. Five grains of this ſolution were then added to 95 grains of water, ſo that 100 grains of water contained $\frac{1}{10}$ grain only, or $\frac{1}{1000}$ of its weight of pure albumen. This was expoſed to the heat of boiling water, and a perceptible opacity was produced in the fluid.

Diſtinguiſhing character of albumen coagulated by heat.

One thouſandth part in water will coagulate;

The effects of the oxymuriate of mercury were next tried. One drop of a ſaturated ſolution of this ſalt, added to 100 grains of water, containing $\frac{1}{1000}$ of its weight of albumen, produced a very evident milkineſs; after ſome hours a curdy precipitate ſeparated and fell to the bottom. A ſolution of half the ſtrength, containing only $\frac{1}{2000}$ of its weight of albumen, was then tried by the ſame re-agent, and even in this inſtance, a ſufficiently obvious effect was produced.

—oxymuriate of mercury precipitates it;

The nitro-muriate of tin is a powerful coagulater of albumen in its unmixed ſtate, but I found it not to be ſo delicate a teſt as the oxymuriate of mercury. One hundred grains of water, containing $\frac{1}{5}$ grain of albumen, *i. e.* $\frac{1}{500}$ of its weight, was not affected by this re-agent until after ſome hours, when the fluid exhibited a degree of milkineſs.

—as does nitro-muriate of tin,

In order to aſcertain the effects of tannin upon albumen, I macerated half an ounce of powdered galls in half a pint of water for ſome hours, and filtered the fluid. A deep brown tranſparent liquor was produced, 100 grains of which I found by evaporation to contain 2½ grains of ſolid reſiduum. Equal parts of this preparation of galls and of a ſolution of albumen, in the proportion of one part to 1000 parts of water, were mixed together; at firſt no effect could be perceived, but after ſome time an evident precipitate was formed and ſlowly ſubſided.

—and tannin.

The aqua lithargyri acetati, or extract of Goulard, is an active precipitant of ſeveral animal fluids; when dropped into a ſtrong ſolution of albumen, a very copious and denſe precipitate is immediately formed.

Goulard throws it down;

It

—but doubtfully.

Aqua lithargyri acetati plentifully precipitates it.

It is, however, ſomewhat difficult to determine how far this effect depends upon the albumen itſelf, as Goulard has been conſidered to be the appropriate teſt of the uncongelable part of the ſerum of the blood, which, it may be ſuppoſed, reſembles the uncoagulable part of the white of the egg. The aqua lithargyri acetati is likewiſe decompoſed by ſeveral of the ſaline bodies which are found to exiſt in almoſt all the animal fluids. I have ſometimes found it to yield a precipitate even when added to diſtilled water, and in all caſes, after expoſure to the atmoſphere for a few hours, the water to which it has been added becomes turbid, and is covered with a thin film. In order to try the effect of this re-agent on albumen, I added one drop of it to 200 grains of water, and upon obſerving that the tranſparency of the fluid was not affected, a ſingle drop of the ſolution of albumen, of the ſame ſtrength with that mentioned above, was added. It formed a denſe precipitate as it fell through the fluid, and upon agitation, the whole was rendered ſlightly milky. In this caſe the proportion of the water to the albumen was as 10.000 to 1; to the uncoagulable part of the white of the egg, it would be about as 30,000 to 1.

Nitrate of ſilver alſo has a like effect;

The next re-agent which I employed was the nitrate of ſilver. A ſingle grain of a ſaturated ſolution of this ſalt produced an evident turbidneſs in 100 grains of water, containing $\frac{1}{10}$ gr. of albumen, and after ſome hours a ſmall quantity of a curdy precipitate fell to the bottom of the veſſel. It might, however, be ſuſpected that in this caſe, the effect produced depended upon a quantity of muriate of ſoda contained in the albumen. I found that 100 grains of water, containing only $\frac{1}{100,000}$ gr. of common ſalt was rendered evidently turbid by one drop of the nitrate of ſilver;* but the precipitate which is formed by the nitrate of ſilver acting on the muriate of ſoda is in the form of a greyiſh powder, and ſubſides more rapidly than it did in the former inſtance, where it produced a white, flaky precipitate.

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* I weighed very exactly a grain of ſalt, and diſſolved it in 200 grains of water. One grain of this ſolution was afterwards added to 99 grains of water, and by repeating the operation for three ſucceſſive times, I obtained 100 grains of water, containing only $\frac{1}{20,000}$ of its weight of ſalt. I then took 99 grains of diſtilled water, and poured into it one drop of the nitrate of ſilver: after waiting for ſome time, until I was ſatisfied that no effect would take place, I added a ſingle drop of the laſt ſolution of ſalt thus making it $\frac{1}{100,000}$ part of the mixture; a faint but perceptible opacity was almoſt immediately produced.

—and nitro-muriate of gold;

I found that a ſolution of albumen of the ſame ſtrength with that employed in the laſt experiment, was immediately decompoſed by the nitro-muriate of gold. One drop of this metallic ſolution inſtantly produced a denſe white precipitate in 100 drops of water, containing only $\frac{1}{10}$ of a grain of albumen.

—and alum.

Albumen in a concentrated ſtate is powerfully coagulated by alum; I found however, that this re-agent is not ſo accurate a teſt of its preſence when in a diluted ſtate, as ſome of thoſe which I had already employed: $\frac{1}{5}$ grain of albumen, diſſolved in 100 grains of water, was indeed rendered ſlightly turbid by the addition of a few drops of a ſaturated ſolution of alum, but no precipitate was formed.

Obs. on the ſolution of albumen.

Before I conclude my account of theſe experiments I muſt obſerve, that the ſtrength of the ſolution of albumen was in all caſes rather leſs than my eſtimate. When I added the albumen to the water, a ſmall portion of it always remained inſoluble, and this was ſeparated from the fluid by filtration before the experiments were performed. This inſoluble part I ſuppoſed to conſiſt of the membranous matter, with which it is ſaid that the white of the egg is interſected. The quantity was indeed almoſt too ſmall to be appreciated, but where it is deſirable to attain as much accuracy as poſſible, I think it neceſſary to mention every circumſtance which may in the ſmalleſt degree affect the reſult.

Remarks. The coagulation by heat is a good diſtinctive character of albumen.

The experiments related above will, I conceive, indicate with a ſufficient degree of accuracy, the preſence of albumen as a conſtituent of an animal fluid. The property of being coagulated by heat is a characteriſtic of this ſubſtance, which will always ſerve as a mark of diſcrimination, and we have found that this property is not deſtroyed by dilution with 1000 times its weight of water. This therefore may be conſidered as a teſt of its preſence minute enough for all practical purpoſes. We have alſo found that there are ſeveral re-agents which poſſeſs the power of precipitating it from its ſolution in water while exiſting only in the ſame proportion. It will be neceſſary, however, to obſerve their operation upon the other nal ſubſtances before we can determine their uſe in the alyſis or compound fluids.

Jelly

Jelly.

Jelly. It is liquifiable by heat and becomes concrete by cold; and is a large component part of animals.

The next ſubſtance which I propoſe to examine is jelly. The peculiar characteriſtic of this body is its property of becoming concrete by cold, and being liquified by the application of a gentle heat. It enters into the compoſition of the blood, though leſs largely than the albumen. It is alſo an ingredient in the ſkin, membranous texture, ligaments, cartilages and tendons. By boiling it is eaſily extracted from theſe ſubſtances, and by evaporation and cooling the whole is reduced to a maſs of greater or leſs ſolidity, in proportion to the previous degree of concentration. By a proceſs of this nature, iſinglaſs is prepared from the bones and cartilages of fiſh; as this ſubſtance has been conſidered to conſiſt of jelly nearly in a ſtate of purity, I employed it for the following experiments.

Obtained by the ſolution of iſinglaſs. One fiftieth part in water will coagulate.

Four grains of iſinglaſs were diſſolved in 200 grains of water, and thus a ſtandard fluid was formed, one grain of which contained $\frac{1}{50}$ gr. of jelly. This ſolution became perfectly concrete by cooling. In the firſt place I wiſhed to aſcertain how ſmall a proportion of jelly diſſolved in water was capable of aſſuming the concrete ſtate. Equal parts of the ſtandard fluid and of water, *i. e.* one part of jelly to 100 parts of water, produced a compound which was completely ſtiffened by cooling; but I found that two parts of water to one of the ſtandard, *i. e.* one part of jelly to 150 parts of water, produced a compound, which though evidently gelatinous, did not aſſume the concrete form.

actively precipitated by tanin.

One of the moſt active precipitants of jelly is the tanning principle. I found that a mixture of 5 grains of the ſtandard ſolution and 95 of water, produced a copious precipitate when added to an equal quantity of an infuſion of galls of the ſame ſtrength with that employed in the experiments upon albumen. In this inſtance the jelly was to the water as 1 to 1000. I afterwards reduced the quantity of jelly until it compoſed $\frac{1}{5000}$ part only of the ſolution, and in this caſe a conſiderable precipitate was ſtill produced by the infuſion of galls.

—but not by aq. lith. acet.

A quantity of the ſtandard ſolution had a few drops of the aqua lithargyri acetati added to it, but no more effect appeared to be produced than would have enſued from mixing it with an equal quantity of pure water.

—nor by ox. m. of merc. nor n. of ſilv. nor n. m. of tin.

No effect was produced by adding the oxymuriate of mercury to the ſtandard ſolution. The nitrate of ſilver and the nitro-muriate of tin were each employed, and produced a very ſlight and almoſt imperceptible opacity.

N. m. of gold gave precip.

The addition of the nitro-muriate of gold cauſed a ſmall quantity of a denſe precipitate when added to the ſtandard ſolution; but when this was ſo far diluted as to contain one grain of jelly in 500 of water, the effect was no longer perceptible.

Mucus.

Mucus; a term not diſtinctly applied by chemiſts.

Animal mucus or mucilage enters largely into the conſtitution of many parts of the body, and forms a conſiderable proportion of ſeveral of the ſecretions. The term mucus had been generally employed in a vague and unreſtricted ſenſe, until Mr. Hatchet, in his valuable paper on the membranous parts of animals, inſerted in the Phil. Tranſ. for 1800, attempted to aſſign to it a more appropriate and definite meaning. He conceives that jelly and mucus are only modifications of the ſame ſubſtance, and do not eſſentially differ from each other; he conſiders it to be entitled to the appellation of mucus, when it is ſoluble in cold water and cannot be brought to the gelatinous ſtate[*]. Dr. Thomſon adopts in general the idea of Mr. Hatchett, and lays down the following as the characteriſtic properties of animal mucilage. It is ſoluble in cold water, inſoluble in alcohol, neither coagulable by heat nor generating into a jelly, precipitable by tan and the nitro-muriate of tin[†]. I have been induced, from the reſults of my obſervations, to form a different opinion reſpecting the relation of jelly and mucus, but I ſhall defer the ſtatement of it until I have related the experiments which have led me to diſſent from ſuch high authority.

Mucus obtained from ſaliva.

By agitating for a ſhort time ſome recent ſaliva in cold water, part of it was diſſolved, and after being paſſed through a filter, was made the ſubject of experiment, being, as I conceived, a ſolution of nearly pure mucus. By a careful evaporation I found that the water had diſſolved $\frac{1}{240}$ part of its weight.

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[*] Hatchett, Phil. Tranſ. 1800. 369 and 381.

[†] nſon, IV. 597.

No

Effect of reagents. No coagulation by moderate heat nor did it gelatinize by cooling.

No effect was produced by the addition of the oxymuriate of mercury, and the nitro-muriate of tin cauſed only a ſlight opacity. No effect was produced by the addition of equal parts of this ſolution and the infuſion of galls. The aqua lithargyri acetati added to the ſolution produced an immediate opacity, and after ſome time a white, fleaky precipitate fell to the bottom of the glaſs. No appearance of coagulation was produced by expoſing the fluid for ſome time to the heat of boiling water, nor was there any tendency to gelatinize by evaporating and afterwards cooling the fluid.

Mucus from an oyſter.

I afterwards endeavoured to obtain mucus in a ſtate of purity from another ſource. For this purpoſe an oyſter was agitated for a few minutes in cold water; the fluid was filtered and appeared ſlightly opake and glutinous.

By evaporation it appeared that it had diſſolved about $\frac{1}{50}$ of its weight of animal matter. A quantity of this ſolution, diluted with an equal bulk of water, was employed in the following experiments.

Reagents.

The oxymuriate of mercury being added to it produced no effect. The infuſion of galls after ſome time produced a ſlight degree of turbidneſs, and at length a precipitate was formed in ſmall quantity. The aqua lithargyri acetati cauſed an immediate opacity, and after ſome time a denſe precipitate.

The Goulard indicates its preſence.

Theſe experiments nearly coincide with the former. In both caſes no effect was produced by the oxymuriate of mercury, thus proving the abſence of albumen. The ſmall precipitate cauſed by the galls indicate the exiſtence of only a very minute quantity of jelly. The effect was ſcarcely as great in this inſtance, where the animal matter compoſed $\frac{1}{100}$ part of the weight of the ſolution, as was produced by the ſame reagent upon a ſolution of jelly, where it compoſed only $\frac{1}{5000}$ part of the weight of the fluid. Very nearly the whole therefore of the animal matter probably conſiſted of mucus, the preſence of which was indicated by the Goulard.

Excluſive teſts; tan for jelly, aq. lith. acet. for mucus, ox. m. of merc. for albumen.

I apprehend that theſe experiments will be deemed ſufficient to eſtabliſh a decided and eſſential difference between mucus and jelly, independent of the gelatinizing property of the latter, the effects produced upon them by the tanning principle and by the aqua lithargyri acetati are exactly oppoſite. Tan is a moſt delicate teſt of jelly, but does not in any degree affect mucus. Aqua lithargyri acetati is a delicate teſt of mucus, but does not in any degree affect jelly. The oxymuriate of mercury, on the contrary, which is one of the moſt accurate teſts of albumen, does not appear to be affected either by jelly or by mucus.

Theſe are the primary and general fluids.

Albumen, jelly, and mucus, I am inclined to conſider as the only primary fluids which are diſperſed through the different parts of the body. Particular veſſels or glands contain and ſecrete particular fluids, which cannot be reſolved into other fluids without decompoſition, as the fibrine of the blood, the reſin of the bile, the urée of the kidney, &c. but theſe are in all inſtances confined to their appropriate organs, and do not neceſſarily enter into the preſent inveſtigation.

Reſumption.

Albumen is known by its coagulability, and precip. by ox. m. merc.

From the above experiments I think we may be entitled to lay down, with a conſiderable degree of accuracy, the leading characteriſtics of the three primary animal fluids, and to eſtabliſh teſts by which their preſence may be minutely aſcertained. The moſt remarkable property of albumen is its becoming coagulated by heat, a property which it retains ſo far as to communicate a degree of opacity to its ſolution in water, when it forms only $\frac{1}{1000}$ part of its weight. A ſolution of the ſame ſtrength has its albumen precipitated by the oxymuriate of mercury, and this teſt will indicate its preſence when compoſing no more than $\frac{1}{2000}$ of the mixture. The tanning principle, the aqua lithargyri acetati, the nitrate of ſilver, and the nitro-muriate of gold, are all teſts of the preſence of albumen nearly as minute as the oxymuriate of mercury, but they are leſs valuable, becauſe their effects are not confined to albumen. The nitro-muriate of tin and alum are alſo precipitants of albumen, but they are leſs delicate in their operation than the reagents enumerated above.

Jelly is known by its concretion on cooling and its precip. by tan.

The peculiar characteriſtic of jelly is its property of becoming concrete by cold, and being again rendered fluid by a gentle heat: we have found that its ſolution in water retains this property when it compoſes $\frac{1}{100}$ part only of the weight of the fluid. Tan is a ſtill more minute teſt of jelly than of albumen, but jelly is not in the leaſt degree affected by the oxymuriate of mercury, and may thus in all caſes be eaſily diſtinguiſhed from it. No effect is produced in jelly by Goulard, and ſcarcely any by the nitrate of ſilver, and the nitro-muriate of tin, when it is in a ſtate of ſuch dilution. By means of tan, jelly may be eaſily detected in a fluid of which it forms only $\frac{1}{5000}$ part.

M[illegible] negative as to the preceding characters; but precip. by aq. lith. acet.

The properties of mucus are principally negative; it is not coagulable by heat, nor capable of becoming gelatinized; it is not precipitable either by the oxymuriate of mercury or by tan, but it may be detected with conſiderable minuteneſs by the aqua lithargyri acetati.

Other teſts.

It appears therefore that the oxymuriate of mercury, tan, and the aqua lithargyri acetati are the three moſt valuable teſts. The nitro-muriate of tin is a leſs delicate teſt of albumen than the oxymuriate of mercury, and is alſo in ſome degree affected by jelly. The nitrate of ſilver appears to be a very nice teſt of albumen, but it is objectionable in conſequence of its being decompoſed by the muriate of ſoda, a ſalt which is ſuppoſed to exiſt in moſt of the animal fluids. The nitro-muriate of gold is a delicate teſt of albumen, but it likewiſe precipitates jelly.

Order of analyſis.

In the analyſis of a fluid which is ſuppoſed to contain either albumen, jelly, or mucus, the firſt ſtep is to obſerve the effect of the oxymuriate of mercury; if this produce no precipitate, we may be certain that the fluid in queſtion contains no albumen. We muſt next employ the infuſion of galls, and if this alſo cauſe no precipitate, we may conclude that the animal matter held in ſolution conſiſts of mucus alone.

Remark.

I have before remarked, that the ideas which I have formed of the nature of jelly and mucus, and the relation which theſe ſubſtances bear to each other, differ materially from thoſe of Mr. Hatchett. It is not indeed without a degree of diffidence that I diſſent from ſo diſtinguiſhed a chemiſt; but I conceive that I am juſtified by the experiments related in this eſſay.—Mr. Hatchett, in the valuable paper to which I have already referred, ſpeaks of the white of the egg as conſiſting of pure albumen; but I believe that in this particular he will be found not perfectly accurate.

Animal mucus reſembles veget. gum.

There is a great reſemblance between the mechanical properties of animal mucus and vegetable gum, and I found that they ſtrongly reſemble each other alſo in their chemical qualities. A ſolution of gum arabic, containing one grain of [illegible]m to 200 grains of water, was not affected either by the o[illegible]riate of mercury or by tan. With the nitro-muriate of tin a[illegible]th the nitrate of ſilver there was only a ſlight degree of op[illegible] but with the aqua lithargyri acetati there was a denſe pre[illegible]te inſtantly formed.

V.

On muſcular Motion. By ANTHONY CARLISLE, *Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, November* 8, 1804.

(Concluded from Page 201.)

Cold deſtroys mobility and ſenſation.

THE loſs of motion and ſenſation from the influence of low temperature, accompany each other, and the capillaries of the vaſcular ſyſtem appear to become contracted by the loſs of animal heat, as in the examples of numbneſs from cold. Whether the ceſſation of muſcular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muſcles themſelves, is doubtful; but the known influence of cold upon the ſenſorial ſyſtem, rather favours the ſuppoſition that a certain temperature is neceſſary for the tranſmiſſion of nervous influence, as well as ſenſation.

Hybernating animals not eaſily drowned. Experiment with the hedge-hog.

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was ſubmerſed in water at 48°, and firmly retained there; air-bubbles began inſtantly to aſcend, and continued during four minutes; the animal was not yet anxious for its liberty. After ſeven minutes it began to look about, attempting to eſcape; at ten minutes it rolled itſelf up, only protruding the ſnout, which was haſtily retracted on being touched with the finger, and even the approach of the finger cauſed it to retract. After fifteen minutes complete ſubmerſion, the animal ſtill remained rolled up, and withdrew its noſe on being touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmoſphere of 62°, with its head inclined downwards; it ſoon began to relax the ſphincter muſcle which contracts the ſkin, ſlow reſpirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog ſubmerſed in water at 94°, remained quiet until after five minutes; about the eighth minute it ſtretched itſelf out, and expired at the tenth. It remained relaxed, and extended, after the ceſſation of the vital functions; and its muſcles were relaxed, contrary to [illegible] of the animal drowned in the colder water.

Connection of irritability with reſpiration, &c.

[illegible] irritability of the heart is inſeparably connected with [illegible]tion. Whenever the inhaled gas differs in its properties from the common atmoſphere, the muſcular and ſenſible parts of the ſyſtem exhibit the change: the actions of the heart are altered or ſuſpended, and the whole muſcular and ſenſorial ſyſtems partake of the diſorder: the temperature of animals, as before intimated, ſeems altogether dependant on the reſpiratory functions, although it ſtill remains uncertain in what manner this is effected.

Diſtribution of heat by the blood.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the ſame individual at various times, or in its ſeveral parts, are always connected with the degree of rapidity of the circulation. It is no very wide ſtretch of phyſiological deduction to infer, that this increaſed temperature is produced by the more frequent expoſure of the maſs of blood to the reſpiratory influence, and the ſhort time allowed in each circuit for the loſs of the acquired heat.

Coagulation of that fluid.

The blood of an animal is uſually coagulated immediately after death, and the muſcles are contracted; but, in ſome peculiar modes of death, neither the one, nor the other of theſe effects are produced: with ſuch exceptions, the two phenomena are concomitant.

Heat delays the laſt coagulation and the contractions of the muſcles, &c.

A preternatural increaſe of animal heat delays the coagulation of the blood, and the laſt contractions of the muſcles: theſe contractions gradually diſappear, before any changes from putrefaction are manifeſted; but the cup in the coagulum of blood does not relax in the ſame manner; hence it may be inferred, that the final contraction of muſcles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-veſſels, becauſe ſuch contractions are not general throughout thoſe ſubſtances. The coagulation of the blood is a certain criterion of death. The reiterated viſitations of blood are not eſſential to muſcular irritability, becauſe the limbs of animals, ſeparated from the body, continue for a long time afterwards capable of contractions, and relaxations.

The chemical combinations of living matter are tranſient, and not renewable.

The conſtituent elementary materials of which the peculiar animal and vegetable ſubſtances conſiſt, are not ſeparable by any chemical proceſſes hitherto inſtituted, in ſuch manner as to allow of a recombination into their former ſtate. The compoſition of theſe ſubſtances appears to be naturally of tranſient duration, and the attractions of the elementary materials which form the groſs ſubſtances, are ſo looſe and unſettled, that they are all decompoſed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

Difficulties attending chemical reſearch into theſe objects.

An extenſive diſcuſſion of the chemical properties attaching to the matter of muſcle would be a labour unſuited to this occaſion; I ſhould not, however, diſcharge my preſent duty, If I omitted to ſay, that all ſuch inveſtigations can only be profitable when effected by ſimple proceſſes, and when made upon the raw materials of the animal fabric, ſuch, perhaps, as the albumen of eggs, and the blood. But, until by ſynthetical experiments the peculiar ſubſtances of animals are compoſed from what are conſidered to be elementary materials, or the changes of organic ſecretion imitated by art, it cannot be hoped that any determinate knowledge ſhould be eſtabliſhed upon which the phyſiology of muſcles may be explained. Such reſearches and inveſtigations promiſe, however, the moſt probable ultimate ſucceſs, ſince the phenomena are neareſt allied to thoſe of chemiſtry, and ſince all other hypotheſes have, in their turns, proved unſatisfactory.

Facts and Experiments tending to ſupport and illuſtrate the preceding Argument.

Temperature of the primary fluids in different animals.

The horſe.

An emaciated horſe was killed by dividing the medulla ſpinalis, and the large blood-veſſels under the firſt bone of the ſternum.

The temperature of the flowing blood wa	103°
Spleen - -	103
Stomach - -	101
Colon - -	98
Bladder of urine	97
Atmoſphere -	30.

Pigs.

Three pigs, killed by a blow on the head, and by the immediate diviſion of the large arteries and veins, entering the middle of the baſis of the heart, had the blood flowing from theſe veſſels of 106, 106½, and 107°; the atmoſpheric temperature being at 31°.

An ox.

An ox, killed in a ſimilar manner, the blood 103; atmoſphere 50°.

Sheep.

Three ſheep, killed by dividing the carotid arteries, and internal jugular veins; their blood 105, 105, 105½°; atmoſphere 41°.

Frogs.

Three frogs, kept for many days in an equable atmoſphere at 54°; their ſtomachs 62.°

Fluid of dropſy.

The watery fluid iſſuing from a perſon tapped for dropſy of the belly 101°: the atmoſphere being 43°, and the temperature of the ſuperficies of the body at 96°.

Theſe temperatures are conſiderably higher than the common eſtimation.

Experiment to ſhew that the volumes of muſcles are increaſed during action.

A man's arm being introduced within a glaſs cylinder, it was duly cloſed at the end which embraced the head of the humerus; the veſſel being inverted, water at 97° was poured in, ſo as to fill it. A ground braſs plate cloſed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus including the arm, was again inverted, ſo that the barometer tube became a gage, and no air was ſuffered to remain in the apparatus. On the ſlighteſt action with the muſcles of the hand, or fore-arm, the water aſcended rapidly in the gage, making librations of ſix and eight inches length in the barometer tube, on each contraction and relaxation of the muſcles.

Crimping of fiſh cannot be effected after rigidity of death.

The remarkable effects of crimping fiſh by immerſion in water, after the uſual ſigns of life have diſappeared, are worthy attention; and whenever the rigid contractions of death have not taken place, this proceſs may be practiſed with ſucceſs. The ſea fiſh deſtined for crimping are uſually ſtruck on the head when caught, which, it is ſaid, protracts the term of this capability; and the muſcles which retain this property longeſt are thoſe about the head. Many tranverſe ſections of the muſcles being made, and the fiſh immerſed in cold water, the contractions called crimping take place in about five minutes; but, if the maſs be large, it often requires thirty minutes to complete the proceſs.

Experiment. Two fiſh were ſcored and one of them crimped. Its ſpecific gravity, and alſo its abſolute weight, was increaſed by crimping.

Two flounders, each weighing 1926 grains, the one being in a ſtate for crimping, the other dead and rigid, were put into water at 48°, each being equally ſcored with a knife. After half an hour, the crimped fiſh had gained in weight 53 grains, but the dead fiſh had loſt 7 grains. The ſpecific gravity of the crimped fiſh was greater than that of the dead fiſh, but a quantity of air-bubbles adhered to the ſurfaces of the crimped muſcles, which were rubbed off before weighing; this gas was not inflammable.

The ſpecific gravity of the crimped fiſh - - 1,105
of the dead fiſh, after an equal
immerſion in water - - 1,090.

So that the acceſſion of water ſpecifically lighter than the muſcle of fiſh, did not diminiſh the ſpecific gravity of crimped muſcle, but the contrary: a proof that condenſation had taken place.

Other ſubſtances crimped with like effect.

A piece of cod-fiſh weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another leſs vivacious piece, of fifteen pounds, gained one ounce and half*.

The hinder limb of a frog, having the ſkin ſtripped off, and weighing $77\frac{1}{10}$ grains, was immerſed in water at 54°, and ſuffered to remain nineteen hours, when it had become rigid, and weighed $100\frac{1}{4}$ grains. The ſpecific gravity of the contracted limb had increaſed, as in the crimped fiſh.

Six hundred and thirty grains weight of the ſubſcapularis muſcle of a calf, which had been killed two days from the 10th of January, was immerſed in New River water at 45°. After ninety minutes, the muſcle was contracted, and weighed in air 770 grains: it had alſo increaſed in ſpecific gravity, but the quantity of air-bubbles formed in the interſticial ſpaces of the reticular membrane made it difficult to aſcertain the degree.

Fibrils of muſcles contract by application of water.

Some of the ſmalleſt faſciculi of muſcular fibres from the ſame veal, which had not been immerſed in water, were placed on a glaſs plate, in the field of a powerful microſcope, and a drop of water thrown over them, at the temperature of 54°, the atmoſphere in the room being 57°. They inſtantly began to contract, and became tortuous.

On confining the ends of another fibril with little weights of glaſs, it contracted two-thirds of its former length, by ſimilar treatment. The ſame experiment was made on the muſcular fibres of lamb and beef, twelve hours after the animals had been killed, with the like reſults. Neither but not by acid or ſaline water.

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* I am informed that the crimping of freſh water fiſhes requires hard water, or ſuch as does not ſuit the purpoſes of waſhing with ſoap. This fact is ſubſtantiated by the practice of the London fiſhmongers, whoſe experience has taught them to employ pump water, or what is commonly called hard water.

vinegar, nor water ſaturated with muriate of ſoda, nor ſtrong ardent ſpirit, nor olive oil, had any ſuch effect upon the muſcular fibres.

Cold renders muſcles

The amphibia, and coleopterous inſects, become torpid at 34°. At 36° they move ſlowly, and with difficulty; and, at a lower temperature their muſcles ceaſe to be irritable. The muſcles of warm-blooded animals are ſimilarly affected by cold.

Muſcles of frogs irritable after freezing.

The hinder limbs of a frog were ſkinned and expoſed to cold at 30°, and the muſcles were kept frozen for eight hours, but on thawing them, they were perfectly irritable.

The ſame proceſs was employed in the temperature of 20°. and the muſcles kept frozen for twelve hours, but that did not deſtroy the irritability.

Heat deprives muſcles of their irritability;

In the heat of 100°, the muſcles of cold-blooded animals fall into the contractions of death; and at 110°, all thoſe of warm blood, as far as theſe experiments have been extended. The muſcles of warm-blooded animals, which always contain more red particles in their ſubſtance than thoſe of cold blood, are ſoon deprived of their irritability, even although their relative temperatures are preſerved; and reſpiration in the former tribe is more eſſential to life than in the latter.

as do poiſons,

and electricity.

Many ſubſtances accelerate the ceſſation of irritability in muſcles when applied to their naked fibrils, ſuch as all the narcotic vegetable poiſons, muriate of ſoda, and the bile of animals; but they do not produce any other apparent change in muſcles, than that of the laſt contraction. Diſcharges of electricity paſſed through muſcles, deſtroy their irritability, but leave them apparently inflated with ſmall bubbles of gas; perhaps ſome combination obtains which decompoſes the water.

Experiments on muſcles immerſed in fluids.

The four ſeparated limbs of a recent frog were ſkinned, and immerſed in different fluids; *viz.* No. 1, In a phial containing ſix ounces by meaſure of a ſaturated aqueous ſolution of liver of ſulphur made with potaſh; No. 2, In a diluted acetic acid, conſiſting of one drachm of concentrated acid to ſix of water; No 3, in a diluted alkali, compoſed of cauſtic vegetable alkali one drachm, of water ſix ounces; No. 4, in pure diſtilled water.

The phials were all corked, and the temperature of their contents was 46°.

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muſcles were highly irritable.

The limb in No. 2, after the ſame duration, had become rigid, white, and ſwollen; it was not at all irritable. By removing the limb into a diluted ſolution of vegetable alkali, the muſcles were relaxed, but no ſigns of irritability returned.

No. 3, under all the former circumſtances, retained its previous appearances, and was irritable, but leſs ſo than No. 1.

No. 4 had become rigid, and the final contraction had taken place.

Muſcular irritability deſtroyed by other cauſes.

Other cauſes of the loſs of muſcular irritability occur in pathological teſtimonies, ſome examples of which may not be ineligible for the preſent ſubject. Workmen whoſe hands are unavoidably expoſed to the contact of white lead, are liable to what is called a palſy in the hands and wriſts, from a torpidity of the muſcles of the fore arm. This affection ſeems to be decidedly local, becauſe, in many inſtances, neither the brain, nor the other members, partake of the diſorder; and it ofteneſt affects the right hand. An ingenious practical chemiſt in London has frequently experienced ſpaſms and rigidity in the muſcles of his fore arms, from affuſions of nitric acid over the cuticle of the hand and arm. The uſe of mercury occaſionally brings on a ſimilar rigidity in the maſſeter muſcles.

Leſs blood flows through a contracted muſcle.

A ſmaller quantity of blood flows through a muſcle during the ſtate of contraction, than during the quieſcent ſtate, as is evinced by the pale colour of red muſcles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæſection, when the muſcles of the limb are kept rigid, and the increaſed flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muſcular fibrils takes place during each contraction, and that its free courſe obtains again during the relaxation. This ſtate of the vaſcular ſyſtem in a contracted muſcle, does not, however, explain the diminution of its bulk, although it may have ſome influence on the limb of a living animal.

When

A contracted muſcle is leſs ſenſible.

When muſcles are vigorouſly contracted, their ſenſibility to pain is nearly deſtroyed; this means is employed by jugglers for the purpoſe of ſuffering pins to be thruſt into the calf of the leg, and other muſcular parts with impunity: it is indeed reaſonable to expect, *a priori*, that the ſenſation, and the voluntary influence, cannot paſs along the nerves at the ſame time *.

Moral cauſes influence the muſcles in the human ſpecies.

In addition to the influences already enumerated, the human muſcles are ſuſceptible of changes from extraordinary occurrences of ſenſible impreſſions. Long continued attention to intereſting viſible objects, or to audible ſenſations, are known to exhauſt the muſcular ſtrength: intenſe thought and anxiety, weaken the muſcular powers, and the paſſions of grief and fear produce the ſame effect ſuddenly whilſt the contrary feelings, ſuch as the proſpect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

Mental as well as muſcular actions may by habit become automatic,

and then give little fatigue.

It is a very remarkable fact in the hiſtory of animal nature, that the mental operations may become almoſt automatic, and, under ſuch habit, be kept in action, without any interval of reſt, far beyond the time which the ordinary ſtate of health permits, as in the examples of certain maniacs, who are enabled without any inconvenience, to exert both mind and body for many days inceſſantly. The habits of particular modes of labour and exerciſe are ſoon acquired, after which, the actions become automatic, demand little attention, ceaſe to be irkſome, and are effected with little fatigue: by this happy proviſion of nature, the habit of induſtry becomes a ſource of pleaſure, and the ſame appears to be extended to the docile animals which co-operate with man in his labours.

Voluntary and involuntary muſcles.

Three claſſes of muſcles are found in the more complicated animals. Thoſe which are conſtantly governed by the will, or directing power of the mind, are called voluntary muſcles. Another claſs, which operate without the conſciouſneſs of the mind, are denominated involuntary; and a mixed kind occur in the example of reſpiratory muſcles, which are governed by the will to a limited extent; nevertheleſs the exigencies of the animal feelings eventually urge the reſpiratory movements in deſpight of the will. Theſe laſt muſcles appear to have become automatic by the continuance of habit.

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* I have often obſerved that a ſmall electric ſhock may be received without pain through the muſcles of the fore arm; but I imagined it to be owing to the want of power in ſuch a ſhock to increaſe the contraction.—N.

Voluntary actions require education; the involuntary are cauſed by ſtimuli.

The uſes of voluntary muſcles are attained by experience, imitation, and inſtruction: but ſome of them are never called into action among Europeans, as the muſcles of the external ears, and generally the occipito-frontalis. The purely involuntary muſcles are each acted upon by different ſubſtances, which appear to be their peculiar ſtimuli; and theſe ſtimuli co-operate with the ſenſorial influence in producing their contractions: for example, the bile appears to be the appropriate ſtimulus of the muſcular fibres of the alimentary canal below the ſtomach, becauſe the abſence of it renders thoſe paſſages torpid. The digeſted aliment, or perhaps the gaſtric juice in a certain ſtate, excites the ſtomach. The blood ſtimulates the heart, light the iris of the eye, and mechanical preſſure ſeems to excite the muſcles of the œſophagus. The laſt cauſe may perhaps be illuſtrated by the inſtances of compreſſion upon the voluntary muſcles, when partially contracted, of which there are many familiar examples. Probably the muſcles of the oſſicula auditûs are awakened by the tremors of ſound; and this may be the occaſion of the peculiar arrangement obſervable in the chorda tympani, which ſerves thoſe muſcles.

Stimuli ſeem to excite ſenſorial power in ſome reſpect reſembling the paſſions.

Theſe extraneous ſtimuli ſeem only to act in conjunction with the ſenſorial power, derived by thoſe muſcles from the gangliated nerves, becauſe the paſſions of the mind alter the muſcular actions of the heart, the alimentary canal, the reſpiratory muſcles, and the iris; ſo that probably the reſpective ſtimuli already enumerated, only act ſubſerviently, by awakening the attention of the ſenſorial power, (if that expreſſion may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of ſympathetic nerves, is effected without conſciouſneſs: for, when the attention of the mind, or the more intereſting paſſions prevail, all the involuntary muſcles act irregularly, and unſteadily, or wholly ceaſe. The movements of the iris of the common parrot, is a ſtriking example of the mixed influence.

Lower tribes of animals act by external excitements.

The muſcles of the lower tribes of animals, which are often entirely ſupplied by nerves coming from ganglions, appear of this claſs; and thus the animal motions are principally regulated by the external ſtimuli, of which the occurrence ſeems to agree with the animal neceſſities: but the extenſive illuſtrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

The nervous power ſeems conſtantly active through life.

There are two ſtates of muſcles, one active, which is that of contraction, the other, a ſtate of ordinary tone, or relaxation, which may be conſidered paſſive, as far as it relates to the mind; but the ſenſorial or nervous power ſeems never to be quieſcent, as it reſpects either the voluntary or involuntary muſcles during life. The yielding of the ſphincters appears to depend on their being overpowered by antagoniſt muſcles, rather than on voluntary relaxation, as is commonly ſuppoſed.

I have now finiſhed this endeavour to exhibit the more recent hiſtorical facts connected with muſcular motion.

Concluſion.

It will be obvious to every one, that much remains to be done, before any adequate theory can be propoſed. I have borrowed from the labours of others, without acknowledgement, becauſe it would be tedious to trace every fact, and every opinion to its proper authority many of the views are perhaps peculiar to myſelf, and I have adduced many general aſſumptions and concluſions, without offering the particular evidence for their confirmation, from a deſire to keep in view the remembrance of retroſpective accounts, and to combine them with intimations for future reſearch. The due cultivation of this intereſting purſuit cannot fail to elucidate many of the phenomena in queſtion, to remove premature and ill-founded phyſiological opinions, and eventually to aid in rendering the medical art more beneficial, by eſtabliſhing its doctrines on more extenſive and accurate views of the animal economy

VI.

On the Meaſure of Mechanic Power. In a Letter from Mr. J. C. Hornblower.

To Mr. NICHOLSON.

Dear Sir,

Meaſure of mechanic power or effect.

I AM glad to find that ſomebody has ſeconded my motion concerning *horſe power*, and I hope the ſubject will not be diſmiſſed

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miſſed until it has paſſed the unanimous aſſent of both theoretic and practical mechanics; and I muſt here expreſs my acknowledgments to Mr. Gregory for his improved method of deciding the queſtion. It is of abſolute importance, that the draught be in a circular direction, and alſo that the radius of the circle be given; for no poſition was ever more demonſtrable than that the leſs the radius the leſs can a horſe (whoſe ſides are equal) exert his faculty of traction.

But I muſt beg leave to uſe a little freedom (in no wiſe unbecoming, I hope,) in adverting to what Mr. Gregory in conjunction with Profeſſor Robiſon has advanced on the ſubject of Mr. Smeaton's mode of defining *mechanic powers* and *mechanical effect*; and I am ſurpriſed that among men of talent and aſſiduity there can be a difference of opinion. It ſeems to me, that if any reaſon can be found, it muſt be that we do not underſtand the ſubject, and, perhaps I may give a decided proof of it in what I ſhall advance concerning it.

However, I am ſure that when a ball of caſt iron, of twenty inches diameter is elevated by means of a pinion and wheel connected to the ſides of a pair of ſheers, and left to hang there a little while,—if I cut the rope that ſuſtains it, it will fall freely a certain height, in a certain portion of time, and would daſh a faulty cylinder of a ſteam engine in pieces. And I am ſo well ſatisfied that there was a certain tendency in this ball to fall towards the centre of the earth, that I need not take a moment to examine the truth of it; but that the deſtruction of the cylinder was occaſioned by the ball falling from the point to which it was raiſed, (how it was raiſed is no part of the ſubject.) Then I ſay, that if the ball had not fallen from the height it was, or if the ball had not been ſo heavy as it was, or if it had had its velocity retarded by any means, the cylinder would not have been broken. I will add, that had this ball been a true ſphere, and the cylinder had not been there, but a certain curved ſurface in its place, which ſhould have received the ball to prevent it impinging in the line of deſcent, it would have been turned out of its courſe conformably to the nature and poſition of the curve: and all that is above common apprehenſion in this matter, is, that while the ball was falling, its velocity was increaſing every inſtant, and that when it met with the curve (if it gave it an horizontal courſe) it would proceed with uniform velocity juſt

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juſt twice the ſpace through which it fell, allowing the ſame time for this horizontal courſe as it had in falling its perpendicular height, and that its tendency is to continue that courſe for ever.

From this ſtatement of the law of bodies in motion, (ſo far as it goes) I think we need not be very diffident in ſaying, that it is from ſimilar facts, (though leſs philoſophically obſerved) that we obtain our primary ideas of motion. The apple falling from the tree is a very good inſtance to the point, and it would not require a very extraordinary ſtretch of genius to apply ſuch an accident as that to any thing like the pile engine, ſtamping preſs, &c.

But there are certain ſpeculative mechanics, who in their mode of accounting for effects like what are here ſtated, have choſen to adopt terms of a very different import, and for ſome latent reaſon wiſh to keep gravitation out of ſight. The writer of the article Dynamics in the Supplement of the Encyclopedia Britannica, calls it preſſure, and by his way of philoſophiſing in the explanation of the meaſure of mechanic power, has (in my opinion) laboured to make it as myſterious as poſſible.

I muſt for the ſake of thoſe of your readers who have not the work to refer to, quote a few paſſages now and then from the above popular work, and here I would refer them to the article on Machinery, where he begins by ſtating, that different notions have been entertained on this ſubject by Leibnitz, des Cartes, and other eminent mechanics of the laſt century, and adds, "*that ſome of the moſt eminent practitioners of the preſent times (for we muſt include Mr. Smeaton in the number) have given meaſures of mechanical power in machinery, which we think inaccurate, and tending to erroneous concluſions and maxims.*"

He then proceeds to explain and demonſtrate the true meaſure of mechanic power, and he begins by ſuppoſing a man preſſing uniformly on a maſs of matter for a certain time; and going on with the ſubject takes occaſion to diſtinguiſh between *the weight of a body* and *its heavineſs!* and, towards the latter end of that ſection he comes to ſome ſort of a concluſion of the ſubject, ſo far as to ſay what is the real meaſure of mechanic power: I ſee I muſt make endleſs quotations if I regard the very letter of his argument, but I hope I ſhall be excuſed; however I will quote the laſt paragraph verbatim.

Relating

Meaſure of mechanic power or effect.

Relating to the preſſure of the man juſt mentioned, he ſays, "but farther we know that this preſſure is the exertion *, we have no other notion of our own force, and our notion of gravity, of elaſticity, or any other natural force is the ſame. We alſo know, that the continuance of this exertion fatigues and exhauſts our ſtrength as completely as the moſt violent motion. A dead pull as it is called of a horſe at a poſt fixed in the ground, is a uſual trial of his ſtrength. No man can hold out his arm horizontally for much more than a quarter of an hour, and the exertion of the laſt minutes gives the moſt diſtreſſing fatigue, and diſables the ſhoulder for action for a conſiderable time after. *Thus is therefore an expenditure of mechanical power in the ſtrict primitive ſenſe of the word.* Of this expenditure we have an exact and adequate effect and meaſure in the quantity of motion produced, that is in the product of the quantity of matter by the velocity generated in it by this exertion. And it muſt be particularly noticed, that the meaſure is applicable even to caſes where no motion is produced by the exertion; that is, if we know that the exertion which is juſt unable to ſtart a block of ſtone lying on a ſmooth pavement, but would ſtart it if increaſed by the ſmalleſt addition, and if we know that this would generate in a ſecond 32 feet of velocity in 100 pounds of matter, we are certain that it was a preſſure equal to the weight of this 100 pounds. It is a good meaſure, though not immediate, and may be uſed without danger of miſtake when we have no other."

I ſhould not have quoted ſo much of this ſection, if it had not been that I think it contains an unequivocal interpretation of the writers notion of the true meaſure of mechanic power, and at once exhibiting, in my proud opinion, the fallacy of the doctrine in *toto*. What! ſhall mere muſcular exertion, whether of horſe or man, be eſteemed even an auxiliary to get the conception of the nature of the thing; how much leſs then ſhall it be ſet forth as the thing itſelf? In perfect conſonance with this writer, Mr. Gregory, page 152, Philoſophical Journal, Vol. XI. ſays, ſuppoſe that a horſe while ſtanding ſtill, ſuſtains by means of a rope and ſimple fixed pulley, a maſs of an hundred weight, and thus keeps it ſuſpended at the top of a well for the ſpace of a minute; neither the animal nor the weight moves: but ſhall we ſay, in conformity

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[*] Article Machinery, Supplement, 4th ſec.

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as it would ſeem with Mr. Smeaton's meaſure, that there is no power expended, no effect produced. On the contrary we know there is a power expended, and that effect, if ſufficiently long continued, would completely tire the horſe. Then let us have a *poſt* inſtead of the horſe, and ſurely that will not tire, and what will be the conſequence then? why then there will be no power expended, and no effect produced; and I beg leave to aſk my opponents, what is the power expended when a horſe or other animal is placed there to ſuſtain the weight? is it any more than the expence of nervous or muſcular action; and has that any analogy with a weight deſcending through a given ſpace, either uniformly or accelerated? and, I aſk again, what is the effect produced more than what is produced by the poſt? the horſe does but keep the weight from dropping into the well, and the poſt will do the ſame; indeed you may ſay, that when you hang up your hat, that the pin which ſuſtains it, prevents it from falling, as does the horſe the maſs in the well, and that therefore there muſt be ſome power expended on the pin.

It is really difficult to be grave on this occaſion; but I feel myſelf reſtrained by the magnitude of the ſubject, and its importance to the community. Profeſſor Robiſon ſays, when a man holds out his arm horizontally, the exertion towards the end of a quarter of an hour gives the moſt diſtreſſing fatigue, and then ſays this is an expenditure of mechanical power, which I ſhall take the liberty to deny for the preſent. But is it ſuch a mechanical power as Smeaton's, or in fine, is it a power made up of a maſs of matter moving with any determinate velocity either uniform or accelerated? If the learned Profeſſor intended to familiarize the doctrine to people of common ſenſe, he could not have choſen a more indirect and perplexing example.

But let us attend a little further to the ſubject in the fourth ſection of the article Machinery, Sup. Ency. Brit. There he ſays, that "when a man ſupports a weight for a ſingle inſtant, he certainly balances the preſſure or action of gravity on that body," by the way here is a great want of preciſion in the expreſſion, "*preſſure or action of gravity*," as if they were ſynonimous terms, whereas *preſſure* certainly denotes *repulſion*, if the term will bear any definition at all, and to explain the term *gravity* if it will not admit of *attraction*, I am ſure it cannot be called repulſion, but to proceed, " and he continues this action as long as he continues to ſupport it, and we know, that if this body were at the end of a horizontal arm turning round a vertical axis, *the ſame effort* which the man exerted in merely carrying the weight, if now exerted on the body by puſhing it horizontally round the axis, will generate in it the ſame velocity which gravity would generate by its falling freely."

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A more erroneous propoſition was never introduced to the theory or practice of mechanics. What, is there no difference in a man carrying a load on his ſhoulder, and putting it into a truck? or to come nearer the Profeſſor's propoſition, let the man who has to carry two hundred pounds for one mile be permitted to take the weight from his ſhoulders, and reſt it on the arm of any thing like a horſe wheel, perfectly detached from the mill gear, let the gudgeons be oiled, and then let him "*puſh*" it horizontally round its axis until he has travelled a mile.

Now without aſking the man which he likes beſt, let us ſee what he does by placing his load on the arm of the horſe-wheel, and puſhing it round. Why, he certainly overcomes the additional friction which his load has added to the weight of the wheel, and that is all, and if you will let us have gudgeons which have no friction, the man need not to walk far to puſh the horizontal arm into perpetual motion.

But now for the monſtrous concluſion by this propoſition. " If the man's exertion was employed to generate motion inſtead of counteracting gravity, he would generate during that minute the ſame motion that gravity would; that is 60×32 feet velocity per ſecond in a maſs of 30 pounds. There would be 30 pounds of matter moving with the velocity of 1920 feet per ſecond. We would expreſs this production or effect by 30×1920, or by 57600 as the meaſure of the man's exertion during the minute."

Here is evidently a typographical error, *ſecond* for *minute*, but when we admit ſuppoſitions for the ſake of illuſtration, there ought to be ſome conformity in the ſuppoſition to the fact it is intended to illuſtrate: then I would aſk, where's the man who can generate 32 feet of velocity in a maſs of 30 pounds in one ſecond? to be ſure he can let it fall a ſecond, but he cannot *carry* it 32 feet in a ſecond; but he ſays, " we

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would expreſs the production or effect by 30 × 1920 or 5760 as the meaſure of the man's exertion during the minute." Sir, it is more than even one of Boulton and Watts' horſes can do.—Well may he ſay, "ſuch an exertion will completely exhauſt a man's ſtrength."

He then goes on to conſider "*more narrowly* what a man *really does*, when he performs what Mr. Smeaton allows to be the production of a meaſurable mechanical effect. Suppoſe a weight of 30 pounds hanging by a cord which paſſes over a pulley, and that a man taking this cord over his ſhoulder, turns his back to the pulley, and walks away from it, we know that a man of ordinary force will walk along raiſing this weight at the rate of about ſixty yards in a minute, or a yard in every ſecond, and that he can continue to do this for eight or ten hours from day to day, and that this is all he can do without fatigue. Here are 30 pounds raiſed uniformly 180 feet in a minute, and Mr. Smeaton would expreſs this by 30 × 180 or 5400, and would call this the meaſure of the mechanical effect, and alſo of the expenditure of power. This is very different from our meaſure 57600."—Yes, but I hope not the leſs concluſive on that account.

It is wholly incomprehenſible to me why thoſe men (who have certainly a right to controvert any propoſition which appears to them erroneous) ſhould take up the ſubject, aſſuming points which the doctrine advanced by Mr. Smeaton has nothing to do with. It is clear that all this animal exertion comes at laſt to the law of bodies falling in accelerated velocity, which Smeaton allows to be a diſtinct conſideration, as he ſays, "if the weight deſcends quickly, it is ſenſibly compounded with another law, viz. the law of acceleration by gravity." But how inconſiſtent is it to go about to elucidate the laws of bodies in motion by the action of a man or a horſe. What is the expenditure of animal power but a waſte of what has been uſually termed by anatomiſts nervous ſpirits, or perhaps an inceptive diſorganization of the conſtituent parts of the ligaments and muſcles? in ſhort, we may compare it to contractions, inflammation, and gangrene, but never to the momentum acquired by a body moving through a certain ſpace in a certain time. It is remarkable, that all this reaſoning is about that of which Mr. Smeaton has never ſaid a word, except in his illuſtration of the mechanic power neceſſary to give velocity to heavy bodies, where he ſuppoſes a man puſhing an iron ball on an extended plane, and this not to try his muſcular force, or to ſee what he can do without tiring, but merely in elucidation of the doctrine he ſets out with.

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I have not time at preſent to purſue the ſubject to the extent I wiſh, and to enter on the ground of the miſtake of this great man: nor let it be imagined for a moment that I have availed myſelf of his everlaſting abſence, to call in queſtion what he has advanced in refutation of a ſuppoſed error. It would have remained as it was unto a diſtant period, had it not been, that I ſee Mr. Gregory advancing the ſame opinion, which no doubt he will defend or deſert.

I am, Dear Sir,

Your obedient Servant,

J. C. HORNBLOWER.

If Mr. Gregory ſhould read this, I ſhall be glad if he will ſet me right as to the identity of *animal exertion* and *mechanic power*.

VII.

An Inveſtigation of all the Changes of the variable Star in Sobieſki's Shield, from five Years Obſervations; exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures reſpecting unenlightened heavenly Bodies. By EDWARD PIGOTT, *Eſq. Abridged from the Philoſophical Tranſactions for* 1805.

THE author begins his memoir with an inveſtigation of the periods of change in the variable ſtar in Sobieſki's ſhield, of which the right aſcenſion was 279° $9\frac{1}{2}'$, and its declination ſouth 5° 56′, for end of June 1796. Its rotation on its axis in 1796 was eſtimated at $62\frac{3}{4}$ days, from a mean of ſix obſervations of its greateſt and leaſt brightneſs. In the preſent paper he gives about 26 ſimilar determinations, moſt of them the reſults of very accurate obſervations made in the year 1796, 1797, 1798, 1799, and 1801. From all theſe reſults it

The variable ſtar in Sobieſki's ſhield, revolves on its axis in $61\frac{1}{2}$ days.

it was found that the diſagreements between the periods of change, as deduced from its full brightneſs, were much greater than thoſe deduced from its leaſt illumination. The former gave the mean period 63 days, and the latter 59¾; and the mean of theſe, namely 61½ days, agreeing with the former determination to ¼ day, is as near as could be expected in obſervations of this nature.

The places of full and leaſt brightneſs do not equally divide the ſtar's circumference; they divide it as 7 to 8.

The author in the next place proceeds to examine ſome other of the changes to which this ſtar is ſubject. By tabulated obſervations through the ſame ſeries of years, he finds that the time of decreaſe, from the middle of its full brightneſs to the middle of its leaſt, is on a mean of 34 days; and that the time of its increaſe, from the middle of its leaſt brightneſs to the middle of its full, is in like manner only 27 days. The ſum of theſe numbers amounting to the period 61, ſhews their probable exactneſs. Theſe compared and combined with the former determinations of 1796, give a mean of the whole 33 + and 29 − days. As it thus appears that the time of the decreaſe is longer than that of the increaſe, it follows of courſe, that the places of the full and leaſt brightneſs are not ſituated at the diſtance of half the circumference from each other; and the like circumſtance Mr. P. affirms is found to be the caſe with moſt, if not all the variable ſtars.

Other variable ſtars are ſimilarly affected.

The luminous parts are themſelves variable.

The next particulars to be reviewed were the durations of its brightneſs without any perceptible change, while at its maximum and minimum. Theſe determinations required a tolerable ſucceſſion of obſervations; where that is not the caſe he has omitted them in his tables. From theſe it is found in general, that when the degree of brightneſs at its maximum is leſs than uſual, and its minimum not much decreaſed, the changes take place but very ſlowly, and cannot be ſettled with much accuracy unleſs the obſervations have been made frequently and with great attention. He accordingly paſſes again over the ſeries of years, ſhewing the dates of its magnitudes when at its full brightneſs, and alſo when at its leaſt brightneſs, and he tabulates at the end of the firſt part of his paper, all the different changes that have been examined. The words in the firſt column or compartment deſcribe them; in the ſecond column the preſent reſults are exhibited; in the third are the reſults of the former obſervations; and in the laſt column is placed a mean of both computed proportionally, according to the number of obſervations of each.

TABLE

TABLE VIII.

Table of the general affections of the variable ſtar.

	Days.	Days.	Days, on a mean.
Rotation on its axis - - -	61½	62¾	62−
Duration of brightneſs, at its maximum, without any perceptible change - - - -	8+	11	9½
Ditto, when it does not attain its uſual brightneſs - - - -	20−	—	—
Duration of brightneſs at its minimum, without any perceptible change - - - -	9−	9	9
Ditto, when it does not decreaſe ſo much as uſual - - -	20−	—	—
Decreaſe in time, from the middle of its full brightneſs to the middle of its leaſt - - - - -	34	[illegible]	33+
Increaſe in time, from the middle of its leaſt brightneſs to the middle of its full - - -	27+	3[illegible]	29−
Extremes of its different degrees of brightneſs; with a mean of its uſual variations -	[illegible]+ [illegible] [illegible]	5+ 7[illegible]	[illegible]

The changes in the luminous part, occaſion the periods ſingly taken to differ from each other.

The author having thus ſettled with conſiderable preciſion theſe eſſential variations of the ſtar, proceeds to examine ſome of its other phenomena, particularly one which is common to moſt of the changeable ſtars, and likewiſe in ſome degree to our ſun, namely, that the times of their periodical returns of brightneſs are in general irregular,—a circumſtance ſo intereſting as to engage our attention, and which induced Mr. P. to make the ſucceſſion of obſervations, in the hope of diſcovering its nature and cauſe. With this view he has proceeded to tabulate the ſeries of years, ſo as to aſcertain the apparent rotations in days from the obſerved middle times of its full brightneſs, and alſo from the obſerved middle times of its leaſt brightneſs, for ſingle periods. From theſe it appears, that the periodical returns of brightneſs are uncommonly fluctuating, and that the differences between the extremes are very conſiderable; to account for which he offers the following explanations, [illegible] previouſly, a few plauſible conjectures, and ſome infer[illegible] ariſing from the obſervations themſelves *.

Aſſumed poſitions. 1. The ſtars are opake. 2. They have regular rotations. 3. Their luminous appearance is cauſed by an atmoſphere like that of the ſun.

1ſt. That the body of the ſtars are dark and ſolid.

2d. Their real rotations on their axes are regular.

3d. That the ſurrounding medium is by times generating and abſorbing its luminous particles in a manner nearly ſimilar to what has been lately ſo ingeniouſly illuſtrated by the great inveſtigator of the heavens, Dr. Herſchel, with regard to the ſun's atmoſphere.

4. The luminous parts are ſparingly diſperſed in the ſtar here treated of:

4th. That theſe luminous particles are but *ſparingly diſperſed* in the atmoſphere ſurrounding the variable ſtar of Sobieſki, appears from the ſtar being occaſionally diminiſhed to the 6.7 magnitude, and much leſs. July 4, 179[illegible], it was of the 7th; September 15, 1798, and Auguſt 9, 1803, *of the 9th, if not inviſible.* (See Table VII †.) Does not this indicate a very ſmall portion of light on its *darkened hemiſphere?*

5. Probably ſmall patches:

5th. And may we not with much plauſibility conſider them as ſpots, ſomewhat circular, or of no great extent? for even on its *brighteſt hemiſphere* the *duration* of its full luſtre is, on a mean, only 9½ days of the 62, or about one-ſixth and ½ of its circumference. (See Table VIII. page 110.) The dimenſions therefore of the parts enlightened ſeem much circumſcribed, and can be tolerably eſtimated, and conſequently may be repreſented very ſmall, particularly if the *powerful effect of a little light* and *the length of time* a bright ſpot is remaining in view be taken into conſideration.

6. Changeable in their nature:

6th. And a further ground of preſumption that thoſe principal bright parts are but ſlight patches is, that they undergo *perpetual changes*, and alſo that ſuch changes are very viſible to us, for moſt probably they would be imperceptible, were not the bright parts contraſted by conſiderable intervals or diminutions of light.

7. And deducible from the phenomena.

7th, and laſt. We may obtain ſome idea of the *relative ſituation or intervals between* theſe bright parts, by the obſervations of the increaſe and decreaſe of brightneſs, as thereby the changes and times elapſed are pointed out. (See Table V. page 136; and Phil. Tranſ. for 1797.)

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* The reſt of the paper is given without abridgement, and the author himſelf ſpeaks in the firſt perſon

† The author refers to his tables in the Tranſactions, of which the abridged reſult has been here given.

Experiment with a ſphere.

I have tried practically the effect of the above ſuppoſitions, by placing ſmall white ſpots on a dark ſphere, which being revolved round repreſented the various changes as nearly as could be expected: proceeding therefore with theſe and other conſiderations, I ſhall make ideal drawings of the ſtar with the ſmall illuminated parts in its atmoſphere, and apply to them ſome of the actual obſervations from both the preceding tables, having always in view that each period may, more or leſs, require a different diſpoſition of ſpots, in conſequence of their conſtant changeability.

1ſt *View.*

View of the ſtar at its greateſt brightneſs.

Plate XIII. Fig. 1, A B, the ſtar's polar axis, round which its rotation takes place in 62 days from C to D.

C D, its equator, the 360 degrees of which being revolved in 62 days, gives nearly 5¾ degrees for each day's motion; the brighteſt part or ſpot is repreſented as centrally facing us, and accordingly ſhewing the ſtar in its greateſt luſtre. Were this bright ſpot and the other parts to remain *unchangeable*, they would after having completed the revolution of 360 degrees, or 62 days, (the ſtar's rotation on its axis,) appear again as at firſt, and at every return continue to give exact periodical times, as was nearly the caſe in 1799, between Auguſt and October, (ſee Table IX. p. 142); but if the ſpot becomes obſcure, and another brightens up in a different place, this latter will make the ſtar appear at its next full ſplendour either ſooner or later than the real rotation according to its poſition, thus,

2d *View.*

Anomaly from another bright ſpot cauſing an apparently ſhort period.

Fig. 1. A full brightneſs having been ſhewn by the ſame ſpot, it afterwards loſes its light, and another as bright is produced 5 days motion (or 29 degrees) preceding it at E, ſee Fig. 2. This latter, when turned centrally to the earth, will appear 5 *days ſooner* than the former one, now obſcured, (here marked P,) and ſhow the ſtar at its full luſtre, making the rotation 57 days inſtead of 62, which was the caſe in 1796, the obſerved revolution between September 17 and November 13. (See Table IX.)

3d View.

Another cauſing a period apparently long

Fig. 3. We will now apply a caſe of an interval of too great length, that of 72 days: the ſpot *m* alone having ſhewn us the ſtar in its full luſtre, its light diſappears during the revolution, and another brightens forth ten days (or 58 degrees) *following it* at H; when *m* returns to face us again in 62 days it being obliterated, the ſtar will appear obſcured, and not recover its ſplendour until the new brightened part H becomes central, which being *ten days later* than the poſition in which *m* was ſeen, makes the revolution 72 days inſtead of 62, as was obſerved between July 14 and September 21, 1801. (See Table IX.) In the above caſe the alterations took place while behind the ſtar, otherwiſe ſome irregularities would have been perceived, as will later be noticed. The ſame reaſoning with proper alterations will, I apprehend, account for the other revolutions, yet I ſhall ſoon again reſume the ſubject with regard to a *ſeries* of the greateſt irregularities; at preſent let us proceed to take a few views of the intervals of its *leaſt brightneſs*, which, contrary to my expectation, I find much more difficult to explain than thoſe of the full, although the reſults diſagree leſs among themſelves. The darkened face of the ſtar is here repreſented with a few ſmall changeable bright ſpots, placed in general at a proper diſtance, ſo as to keep up an uninterrupted increaſe and decreaſe of light with regard to us, and are alſo made to correſpond with ſeveral other obſervations.

4th View.

Greateſt period explained; between intervals of leaſt brightneſs.

Fig 4 is to explain the greateſt interval of 74 days, between July 4th, and September 16th 1799. (See Table X.) The darkened hemiſphere here exhibited in its *minimum* July 4th, with the following ſpots, *w* nearly gone off, next a ſmall one *l*, then another P of a ſimilar ſize, preceding the centre a day or two, (or a few degrees,) and laſtly a bright one at D, juſt appearing. During the rotation, D loſing its light and the P becoming *much brighter*, the ſtar at its next return in 62 days, when at its firſt poſition, muſt of courſe appear much brighter, (See fig. 5) but by the retiring of *l* and P continues to diminiſh in luſtre till the appearance of ſome large ſpot from the other hemiſphere; which taking place 12 days afterwards will, (when this time is added to the 62 already revolved) make the revolution of 74 days, is required; for a view of a short interval, for the present let that of 56 days be taken between August 21ſt and October 16th 1801. (See Table X.)

5th view.

Leaſt brightneſs.

The leaſt brightneſs or *minimum* is repreſented by fig. 6, when the bright ſpots *y* and *x* at each extremity of the equatorial diameter are mutually but juſt in ſight and a minute one, *r* alone on its ſurface preceding *y* by 6 days motion: *n n*, are other middling ſized ſpots near *x*, but preceding it; they cannot for the preſent be ſeen, being on the oppoſite or bright hemiſphere. The ſpot *x* during the ſtars revolution having loſt its light, and *r* being conſiderably increaſed, the next *minimum* will be between *n n* and *r*, (inſtead of *x* and *y*.) See fig. 7; and by the retiring of *n n* the *diminution* of the ſtar's light will continue to take place only until the re-appearance of *r*, at the place where *y* was, which being 6 days ſooner than the former poſition, (See fig. 6,) reduces the rotation to 56 days. All the foregoing views are from unconnected periods, where only the ultimate returns of each appearance have been attended to; but now, I ſhall examine a long interval with many intermediate changes, that between June 13th, and September 17th 1796, wherein are included the moſt intricate irregularities and viciſſitudes: theſe obſervations are already printed at full length in the Philoſophical Tranſactions for 1797, and therefore can at any time be inſpected: indeed, I then little thought they would ever become of further uſe, but that of ſtating facts, to which, however, I have always been very partial, and particularly ſo, after having experienced the advantage of Maraldi's printed obſervations on the variable ſtar in Hydra, as it was partly by them that I aſcertained the periodical returns of brightneſs of that ſtar, and which flattered me the more, as Maraldi himſelf had been leſs ſucceſsful in the attempt; See Phil. Trans. for 1786. Yet in the preſent Paper I have omitted all ſuch details, being aware they might be thought too voluminous, but hope at ſome future time the Society will honour them with a place in their library.

The firſt ſketch, Plate XIV. repreſents, for June 13, 1796, the comparative ſize of the bright ſpots ſuppoſed to ſurround the ſtar, but here extended at full length; the next eight following

are

Explanation by the figures of the periodical variations and irregularities of light in the ſtar in Sobieſki's ſhield.

are ſpherical views, on an enlarged ſcale, for each quarterly, rotation or leſs, ſhewing the principal changes, as expreſſed in the adjoining remarks, and correſponding with the obſervations; theſe being taken from my printed paper, as already mentioned, are marked in italics. It will be ſeen that the ſpots by which the changes are principally regulated, are placed at equal diſtances, yet intermediate ones might alſo frequently be inſerted without occaſioning any objection, but that of rendering the explanations more complex.

REMARKS ON PLATE III.

Fig. 2. "*June 18th. Full brightneſs Mag. bright 5th,*" before or after which date the ſtar would appear leſs bright, by the ſpot E being removed from the centre, and one of the others out of view.

Fig. 3. "*July 3d,* 15 *days or* ¼ *rotation being elapſed ſince June 18th, 5th Mag, a little decreaſed*" by the removal of the brighteſt ſpot E, the *h* being much leſs.

Fig. 4. "*July 19th,* 16 *days or* ¼ *rotation 5.6 Mag. ſtill decreaſed,*" N being much leſs than *h*, now gone off. *A ſlight minimum.*"

Fig. 5. "*July 27th,* 8 *days of the rotation,* 5 *Mag. rather increaſed*" by the conſiderable increaſe of N ſince four days, with the addition of F, *a ſlight full brightneſs.*

Fig. 6. "*Aug. 3d,* 7 *days of rotation,* 5,6 *Mag. decreaſed* by the going off of N, the F, which is now reappearing, being reduced to much leſs than F.

Fig. 7. "*Aug. 19th,* 16 *days or* ¼ *rotation,* 5.6 *Mag. again decreaſed,*" by the removal of F, by E being much leſs, and by the *h* alſo being conſiderably diminiſhed.

Fig. 8. "*Sept 3d,* 15 *days or* ¼ *rotation,* 6 *Mag. ſtill more decreaſed,*" by the *h* being much leſs than E, which is now going off, and N ſcarcely reappearing, *another minimum.*

Fig. 9. "*Sept. 17th,* 14 *days or near* ¼ *rotation,* 5 *Mag. full brightneſs conſiderably increaſed,*" by N having retained its increaſed brightneſs of July 27, and now facing us centrally.

1ſt, Thus are exhibited, the two ſhort intervals of its full brightneſs, one between June 18 and July 27, of 39 days, and the other between July 27 and Sept. 17, of 52 days. See Table IX.

2dly,

Explanation by the figures of the periodical variations and irregularities of light in the ſtar in Sobieſki's ſhield.

2dly, The interval of 46 days between the two *minima* of July 19 and Sept. 3; See Table X.

3dly, The long decreaſe of 38 days between July 27 and Sept. 3, and

4thly, The rapid increaſes of 3 and 14 days between the 19 and 27th of July, and the 3d and 17th of September,

As alſo the other intermediate changes, yet I muſt again repeat, particularly as a few days error may occaſionally proceed from the obſervations, that by theſe ſketches it is not meant to give exact drawings of the ſize, diſtances or alterations of the ſpots, but merely to ſhew how the changes may take place, as, I believe, nothing of the kind has hitherto been offered to the public, either with or without corroborating obſervations; nor do I preſume to think, that the explanations are the only ones or beſt can that be imagined, the more ſo, as they ſolely refer (for greater ſimplicity) to the ſtar's equator, while poſſibly, were the ſpots placed in a northern or ſouthern latitude, or permanent ones near the poles, or were a proper inclination, given to the polar axis, they might be more ſatisfactory: however, the materials themſelves, the *obſervations* and *deductions* will I flatter myſelf ever be acceptable, and contribute to facilitate future conjectures, which from an allowable analogy may extend to ſimilar parts of the ſtarry ſyſtem, with regard to the probability of eſtabliſhing whether any of the moſt *irregular or particular* changes may not *return at fixt periods*, or after a certain number of rotations. I think we can entertain but ſlight hopes of it, owing to the *great fluctuation* of the luminous matter, as ſhewn by the *perpetual varying* of the *apparent* revolutions, magnitudes, &c. See Tab. IX. X. and VII. Still it is natural to ſuppoſe, that ſome parts of the atmoſphere of this ſtar may have a leſs tendency than others to become luminous, ſo as to promote at different times, ſimilar appearances; and indeed that is ſtrongly indicated by the *intervals* of the *minima* being far *more regular* than thoſe of the *full brightneſs*, which, with other reaſons induce us to ſuſpect even that one of its hemiſpheres is leſs favourably conſtituted or qualified, than the other for the generating of theſe particles, although they do occaſionally encroach on both ſides, as appears by the obſervations between June and Auguſt, See Phil. Trans. for 1797, or the eight ſketches of 1796, and likewiſe in 1797, ſee Tab. VII. when during *three months* it was only reduced to the

Changeable ſtars may become permanent;

and others diſappear.

There may be dark ſtars,

perhaps equal in number to the viſible ones.

5 or 6 Mag. by which the degree of brightneſs that ſurrounded it, muſt have been nearly equal; had the cauſes of varying its light then ceaſed, it would ever have continued to appear as an unchangeable ſtar of the 5 or 6 Mag. and ſuch is the caſe of ſeveral others that *formerly have been variables*, but for many years retain a ſteady brightneſs, as β Geminorum, δ Urſæ majoris, α Draconis, and perhaps that in the Swan's breaſt, while others, after *ſhewing their changes*, have *entirely diſappeared*, owing to a total abſorption of light, as the famous one in Caſſiopea, in Serpentarius of 1604, that near the Swan's head, and doubtleſs many more. Does not this induce us to preſume that there are alſo others, that have *never ſhewn* a glimpſe of brightneſs? Laſtly, *new variables* may become ſo at different periods, by an unuſual and partial increaſe or diminution of their bright parts, as not unlikely was the caſe of o Ceti, Algol α Herculis, &c. for theſe ſtars being by times very conſpicuous, their changes, had they been always equally great, might have been eaſily noticed by the ancient aſtronomers, who obſerved only with the *naked eye*. A few lines above, I mentioned the probability that there exiſted *primary* inviſible bodies or *unenlightened ſtars* (if I may be allowed the expreſſion) that have ever remained in *eternal darkneſs*; how numerous theſe may be, can never be known. Would it then be too daring or viſionary to ſuppoſe their numbers equal to thoſe endowed with light? particularly when we take into contemplation the ample ſet of bodies viſible only by reflected rays, that appertain to our own ſyſtem, ſuch as the planets, aſteroides, comets, and ſatellites. Do not theſe, although but of a ſecondary nature, lead us to venture on the foregoing more enlarged conjecture; and moreover to ſuſpect, that the *enlightened ſtars* are thoſe that have already attained the higheſt degree of perfection? granting, therefore, ſuch multitudes do really exiſt, cluſters of them, by being collected together as in the milky-way, muſt intercept all more diſtant rays, and if free from any intervening lights, they would appear as *dark ſpaces* in the heavens, ſimilar to what has been obſerved in the Southern Hemiſphere. That ſo few of theſe obſcure places are perceived, may be attributed to their being obliterated by the preſence either of ſome ſcattered ſtars, or of other ſlight luminous appearances.

Our ſun, though variable, is ſtill very luminous;

I have thus fully inveſtigated the nature of this diſtant ſun; a ſingle one among many millions, and ſcarcely perceptible to the ſight; yet of no leſs importance than our own grand luminary. But ours is ſtill ſupplied abundantly with reſplendent particles, while Sobieſki's variable ſtar has them moſt ſparingly diſperſed over its ſphere: a ſcantineſs that apparently muſt occaſion to its ſurrounding planets, conſtant viciſſitudes of uncertain darkneſs, and repletion of light and heat. How far more enviable ſeems our ſituation; I mean that which we enjoy at preſent: there being ſtrong reaſons to believe, that the ſun's luminous appearance has been at times conſiderably diminiſhed; and I have little heſitation in conceiving that it may alſo be reduced at ſome future period to ſmall patches, and then the apparent irregularities of its periodical rotations, which at preſent are only perceived by the obſervations of trifling dark ſpots, would become evidently conſpicuous, particularly when ſeen at a diſtance as remote as the variable ſtars are from us. But ſuch conjectural flights of fancy cannot too ſoon be dropt. I therefore ſhall conclude with obſerving, that theſe inquiries on the alterations of light of the ſtars have been ſo little diſcuſſed, that it is to be hoped they will not be diſcontinued; and although I have already troubled the Society with many papers concerning ſuch changes, I nevertheleſs propoſe, ere long, having the honour of preſenting them with one more, moſt probably my laſt, on this ſubject.

Though it may become reduced in future ages.

EDW. PIGOTT.

VIII.

Account of a Luminous Meteor. By a CONSTANT READER.

To Mr. NICHOLSON.

SIR,

HAVING found it frequently ſtated, that it might be uſeful, ſhould every one who has a fair opportunity of noting with reaſonable accuracy, the courſe and altitude of meteors, deſcribe their appearances as well as they are able; I ſend you the following account to make what uſe of you pleaſe.

Deſcription of the appearance, courſe, and duration of a luminous meteor.

Laſt night (Sunday the 21ſt) paſſing along the Strand, I ſtopped at the door of the Crown and Anchor, the vacant ſpace before it lately cauſed by the pulling down of houſes

Deſcription of the appearance, courſe, and duration of a luminous meteor.

offering a conſiderable view of the heavens, at that time ſplendid with ſtars; I was looking with attention towards the N. W. when ſuddenly a meteor from about 35° of height, ſhot from the W. by N. It was apparently about the ſize of a tennis ball, perhaps hardly ſo large, it was followed by a ſtream of light which ſeemed in ſpecks, the length of the train was about a degree, that is about twice the apparent diameter of the moon. Its courſe was from North of Weſt towards the North, paſſing about 10° below the   of the Great Bear, which I judge was then about 45° above the horizon. Its motion was majeſtic, by no means rapid, I am ſure it was full ten ſeconds in motion, the light not ſo piercing as that of a ſtar of the firſt magnitude, but exceeded that of the ſecond, with which I had full opportunity of comparing it. It ran through 30° of the heavens, deſcribing an arch of great diameter, its path was convex above, and declining downwards. The extinction of it was at an altitude of about 25° having fallen certainly not more than 10°, I do not think ſo much. It very viſibly ſtopped before it was extinguiſhed. It burſt at laſt with very few ſparks, and its train and itſelf together diſappeared in a moment. I had perfect leiſure and ſpace to obſerve its whole courſe, it expired below the ſecond pointer, I inſtantly drew out my watch, and comparing it this morning with the clock of St. Paul's Cathedral, it was exactly at thirty-one minutes after eleven that I obſerved the end of the phenomenon.

I am, Sir,

Your moſt obedient humble Servant,

A CONSTANT READER.

Monday, July 22d, 1805.

IX.

Precipitation of Platina as a Covering or Defence to poliſhed Steel, and alſo to Braſs. In a Letter from Mr. J. STODART.

To Mr. NICHOLSON.

DEAR SIR,

Platina is taken from its ſolution by ether.

YOU kindly favoured me, by inſerting in the laſt Number of your excellent Journal, an account of a method I have uſed with ſucceſs, for gilding poliſhed ſteel with gold; perhaps it may be worth knowing, that a very ſimilar proceſs may be performed with platina. That metal, in a ſtate of ſolution, is taken up from the acid by agitation with ether, in the way that gold is, though certainly with leſs avidity. The ethereal ſolution of platina afforded by this proceſs, is, like that of gold, depoſited on the ſurface of poliſhed iron, or ſteel, forming a coat of defence from ruſt. It is perhaps a fact of equal importance, that the ſurface of poliſhed braſs is coated with platina by the ſame operation that ſteel is; namely, by plunging the braſs for an inſtant into the ethereal ſolution. As far as I know, theſe facts have not hitherto been noticed: on the contrary, authors highly reſpectable, have from ingenious and well conducted experiments, been led to concluſions very oppoſite to thoſe I have advanced. Dr. Lewis, to whoſe genius and induſtry the arts are much indebted, ſays, "gold is the only one of the known metals which the ether takes from acids; and hence this fluid affords a ready method of diſtinguiſhing gold, contained in acid ſolutions." The ſame author gives the following experiment. "Sulphuric ether was poured into a ſolution of platina, and into a compoſition of platina and gold. The vials were ſtopt and ſhaken, the ether received no colour from the ſolution of platina, but became inſtantly yellow from that of the platina and gold." The only way in which I can account for theſe reſults, ſo contrary to my experience, is by ſuppoſing that the platina with which Dr. Lewis made his experiments, was not ſo pure as that with which we are now furniſhed. What I uſed was part of an excellent malleable bar, its ſpecific gravity I do not exactly know. I am inclined to think it was quite pure. The ether was furniſhed by my friend Mr. Hume, whom I am again happy to thank for his kind and able aſſiſtance. The ethereal ſolution of platina is of a beautiful pale yellow colour, does not at all ſtain the hand, and is precipitated by volatile alcali. The precipitate I have not examined. It may be fulminating, and I have no reliſh for exploſions. The coat of platina on ſteel is of a dull white colour. I have no doubt of its proving quite as good a defence from ruſt, as the coat of gold. It is, however, by no means ſo beautiful; for which reaſon a preference will probably be given to the laſt named metal. I have uſed both the gold and platina in coating

and coats iron or ſteel;

and alſo braſs.

Dr. Lewis's experiments gave a contrary effect;

probably becauſe his platina was impure.

The platina coating is leſs beautiful than gold.

different parts of the ſame inſtrument. The effect produced by the contraſt of colour is very beautiful. Whether any of theſe obſervations may be worth communicating through the medium of your moſt uſeful Journal, is a queſtion I beg leave to ſubmit entirely to your judgment. I have not tried any of the eſſential oils with ſolution of platina; further experiments will probably be made with theſe metallic ſolutions, by thoſe who have more time, and a better knowledge of theſe ſubjects. Such purſuits, when the reſults are frankly communicated, promiſe to benefit ſcience, and muſt ultimately prove uſeful to ſociety.

I remain with much reſpect,
Dear Sir, your obliged Servant,
J. STODART.

Strand, July 24, 1805.

X.

Experiments on Wootz. By Mr. DAVID MUSHETT. *From the Philoſophical Tranſactions*, 1805.

(Concluded from Page 204.)

Forging No. 2.

Appearances on forging the cake of woots, No. 2.

ONE half of this cake was heated to a ſcarlet ſhade, and put under the cutting chiſſel; it was at firſt ſtruck lightly, then reheated, and cut comparatively ſoft; but a ſmall crack had over-run the progreſs of the chiſſel. Its ſoftneſs in cutting was attributed to an evident want of ſolidity. The other half cake felt harder under the hammer, but proved afterwards ſpongy throughout the maſs. In the act of cutting, a looſe pulveriſed matter was diſengaged from ſome of the cells, poſſeſſed of a ſhining appearance.

The fractures obtained in conſequence of the diviſion of the half cakes, preſented a flattiſh cryſtallized appearance, more reſembling very white caſt iron, than ſteel capable of being extended under the hammer. One of the middle cuts was entirely cellular with cryſtallized interiors, and incapable of drawing; the correſponding cut of the other half cake was drawn into a ſtrait bar three quarters of an inch in breadth, and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one ſhade lighter than No. 1, it tore leſs out, was equally yolky, and poſſeſſed on the whole an aſpect very unfavourable for good ſteel.

The other two outſide quarters were alſo drawn into ſhape, one under the tilt hammer, and the other by hand. Theſe were more ſolid in the fracture, poſſeſſed fewer ſurface-cracks, ſtood a higher degree of heat, tore out more, and exhibited a ſilky gloſſy grain, at leaſt two ſhades lighter in the colour than the centre pieces.

Forging 3d Cake.

Appearances on forging the cake of wootz, No. 3.

One half of this cake, firſt ſubjected to be cut, was found ſofter than any of the preceding, and exhibited no ſymptom of cracking. The other half was cut at three heats, but found looſe and hollow in the extreme. A conſiderable portion of the ſame brilliant powder, formerly noticed, was here again diſengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulvereſcent ſtate, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumſtance led me to examine every pore and cell throughout the whole fragments. On the upper ſurface of two of them I found ſmall pits containing a portion of the ore, which had been ſlightly agglutinated in the fire, but ſtill highly magnetic. The upper ſurface of the preſent cake, cloſe by the gate or feeder, contained a large pit filled with a ſtratum of ſemi-fuſed ore, ſurmounted by a maſs of vitrified matter, which bore evident marks of containing calcareous earth.

Thoſe who have devoted ſufficient attention to the affinities of iron and earths for carbon, will be ſurpriſed to find that, on this particular ſubject, the rude fabricators of ſteel in Hindoſtan have got the ſtart of our more poliſhed countrymen in the manufacture of ſteel.

Two bars of wootz were formed from this cake, and theſe in point of quality inferior to any of thoſe formerly produced. The appearance of the metal was more varied, leſs homogeneous, and contained more diſtinct laminæ with ruſty ſurfaces, than either of the two former cakes.

It appeared highly probable, from the obſervations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was ſuch as would have formed a quality of ſteel ſofter than No. 1 and 2; but as ſteel of ſuch ſoftneſs requires a greater heat to fuſe it, than when more fully ſaturated with carbonaceous matter, it is probable that the furnace had not been ſufficiently powerful to occaſion complete fuſion of the whole maſs, and generate a ſteel homogeneous in all its parts.

Forging 4th Cake.

Appearances on forging the cake of wootz, No. 4.

Both halves of this cake cut pleaſantly, and with a degree of tenacity and reſiſtance, mixed at the ſame time with ſoftneſs beyond what was experienced in any of the former cakes. Two quarters of this cake were drawn under the tilt hammer, and one by hand. The reſulting bars were nearly perfect. A ſlight ſcale was obſervable upon the bar, from that quarter which contained the figure. The fracture was ſolid, though not homogeneous as to quality and colour, and it appeared pretty evident, that a conſiderable portion of one ſide through the whole bar was in the ſtate of malleable iron, and of courſe not capable of being hardened. It was a ſubject of conſiderable regret, that the cake the moſt perfect and the moſt tenacious of the whole, in the proceſs of forging, ſhould get an imperfection which rendered it uſeleſs for the perfect purpoſes of ſteel.

Forging 5th Cake.

Appearances on forging the cake of wootz, No. 5.

The firſt half of this cake cut uncommonly ſoft for wootz, but by cracking before the chiſſel ſtill exhibited a want of proper tenacity. The next half cut equally ſoft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, ſometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and ſcale, although the fracture exhibited a fair break of a light blue colour, and the grain was diſtinctly marked, and free from yolks.

General Remarks.

Remarks. Wootz appears to be the product of a pecu-

The formation of wootz appears to me to be in conſequence of the fuſion of a peculiar ore, perhaps calcareous, or rendered highly ſo by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other veſſel or crucible, is equally preſumable, in which the ſeparated metal is allowed to cool; hence the cryſtallization that occupies the pits and cells found in and upon the under or rounded ſurface of the wootz cakes.

liar ore fuſed, and ſuffered to cool in a clay veſſel;

The want of homogenity, and of real ſolidity in almoſt every cake of wootz, appears to me to be a direct conſequence of the want of heat ſufficiently powerful to effect a perfect reduction; what ſtrengthens this ſuppoſition much is, that thoſe cakes that are the hardeſt, *i. e.* that contain the greateſt quantity of carbonaceous matter, and of courſe form the moſt fuſible ſteel, are always the moſt ſolid and homogenious. On the contrary, thoſe cakes, into which the cutting chiſſel moſt eaſily finds its way, are in general cellular, replete with laminæ, and abound in veins of malleable iron.

by an heat not ſufficient for good fuſion;

It is probable, had the native Hindoſtan the means of rendering his caſt ſteel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality, and convenient for thoſe purpoſes to which it is applied.

which is the reaſon why it was not caſt in moulds.

The hammering, which is evident around the feeder and upon the upper ſurface in general, may thus be accounted for. When the cake is taken from the pot or crucible, the feeder will moſt probably be ſlightly elevated, and the top of the cake partially covered with ſmall maſſes of ore and ſteel iron, which the paucity of the heat had left either imperfectly ſeparated or unfuſed. Theſe moſt probably, to make the product more marketable, are cut off at a ſecond heating, and the whole ſurface hammered ſmooth.

I have obſerved the ſame facts and ſimilar appearances in operations of a like nature, and can account ſatisfactorily for it as follows.

The firſt portions of metal, that are ſeparated in experiments of this nature, contain the largeſt ſhare of the whole carbon introduced into the mixture. It follows of courſe, that an inferior degree of heat will maintain this portion of metal in a ſtate of fluidity, but that a much higher temperature is requiſite to reduce the particles of metal, thus for a ſeaſon robbed of their carbon, and bring them into contact with the portion firſt rendered fluid, to receive their proportion of the ſteely principle. Where the heat is languid, the deſcent of the laſt portions of iron is ſluggiſh, the maſs below begins to loſe its fluidity, while its diſpoſition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic maſſes of various diameter, riſing up for half an inch or more into the glaſs that covers the metal; theſe are neatly welded and inſerted into each other, and diminiſh in diameter as they go up. The length, or even the exiſtence of this feeder or excreſcence, depend upon the heat in general, and upon its temperature at different periods of the ſame proceſs. If there has been ſufficient heat, the ſurface will be convex and uniformly cryſtallized; but if the heat has been urged, after the feeder has begun to melt and an affinity eſtabliſhed between it and the ſteeling maſs below, it will only partially diſappear in the latter, and the head or part of the upper end of the feeder will be found ſuſpended in the glaſs that covers the ſteel.

The ſame or ſimilar phenomena take place in ſeparating crude iron from its ores, when highly carbonated, and difficult, from an exceſs of carbon, of being fuſed.

The diviſion of the wootz cake by the manufacturers of Hindoſtan, I apprehend is merely to facilitate its ſubſequent application to the purpoſes of the artiſt; it may ſerve at the ſame time as a teſt of the quality of the ſteel.

Experiments to aſcertain the comparative meaſure of carbon in wootz, by the quantity of lead it reduces from flint glaſs.

To aſcertain, by direct experiment, whether wootz owed its hardneſs to an extra quantity of carbon, the following experiments were performed with various portions of wootz, of common caſt ſteel, and of white crude iron, premiſing that in operations with iron and its ores, I have always found the comparative meaſure of carbon beſt aſcertained by the quantity of lead, which was reduced from flint glaſs.

1ſt Cake.	Grains.
Fragments of wootz - - - - - -	65
Pounded flint glaſs three times the weight -	195

This mixture was expoſed to a heat of 160° Wedgwood, and the wootz fuſed into a well cryſtallized ſpherule of ſteel. A thin cruſt of revived lead was found below the wootz, which weighed 9 grains, or $\frac{138}{1000}$ the weight of wootz.

2d Cake.

	Grains.
Fragments of wootz	80
Flint glaſs, ſame proportion as above	240

The fuſion of the mixture in this experiment was productive of a maſs of lead weighing 10 grains, equal to ⅛th the weight of the wootz.

Experiment to aſcertain the comparative meaſure of carbon on wootz, by the quantity of lead it reduces from flint glaſs.

3d Cake.

Fragments of wootz	75
Flint glaſs	225

The maſs of lead precipitated beneath the ſteel in this experiment, amounted to 9 grains, or $\frac{12}{100}$ the weight of the wootz employed.

4th Cake.

Fragments of wootz	93
Flint glaſs	279

Lead obtained, precipitated from the glaſs by means of the carbon of the wootz 14½ grains, equal to $\frac{156}{1000}$ the weight of the wootz.

5th Cake.

Fragments of wootz	69
Flint glaſs	207

The lead revived in this experiment amounted to 7 grains, which is equal to $\frac{102}{1000}$ the weight of the wootz.

6th. Caſt Steel formed with $\frac{1}{60}$th part of its Weight of Carbon.

Fragments	90
Cryſtal glaſs	270

Lead revived 8½ grains equal to $\frac{94}{1000}$ the weight of the ſteel introduced.

7th. White caſt Iron dropt while Fluid into Water.

Fragments	103
Cryſtal glaſs	309

The fuſion of this precipitated 23½ grains of lead which is equal to $\frac{228}{1000}$ the weight of the caſt iron.

Recapitulation of theſe Experiments.

Recapitulation of the experiments.

1ſt cake of wootz revived of lead -	,139
2d ditto - - -	,125
3d ditto - - -	,120
4th ditto - - -	,156
5th ditto - - -	,102
Steel containing $\frac{1}{60}$ of its weight of carbon	,094
Caſt iron - - - -	,228

Wootz contains more carbon than ſteel does, and leſs than caſt iron.

It would appear to reſult from theſe experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of caſt ſteel in this country, and that ſome particular cakes approach conſiderably to the nature of caſt iron. This circumſtance, added to the imperfect fuſion which generally occurs in the formation of wootz, appear to me to be quite ſufficient to account for its refractory nature, and unhomogeneous texture.

Its ore is probably very excellent.

Notwithſtanding the many imperfections with which wootz is loaded, it certainly poſſeſſes the radical principles of good ſteel, and impreſſes us with a high opinion of the ore from which it is formed.

The poſſeſſion of this ore for the fabrication of ſteel and bar iron, might to this country be an object of the higheſt importance. At preſent it is a ſubject of regret, that ſuch a ſource of wealth cannot be annexed to its capital and talent. Were ſuch an event practicable, then our Eaſt India Company might, in their own dominions, ſupply their ſtores with a valuable article, and at a much inferior price to any they ſend from this country.

XI.

A Memoir on the Webs of Spiders. By C. L. CADET.*

On the natural and medical hiſtory of ſpiders.

SPIDERS have often excited the curioſity of naturaliſts and the attention of phyſicians. The former have ſucceſsfully ſtudied the habits and conduct of theſe inſects; and notwithſtanding the repugnance they naturally inſpire, theſe accounts have become intereſting, from the induſtry with which they extend their webs for ſeizing their prey, and from obſervations on the multiplicity and arrangement of their eyes, which are geometrically diſpoſed on a motionleſs head, in a manner conformable to their neceſſities. Their combats, the ſingularity of their amours, their ſenſibility for muſic, and their patience, all conſtitute ſubjects of wonder in the hiſtory of ſpiders. Phyſicians have examined whether their bite be really venomous, as is generally thought; and they have found only two ſpecies productive of danger, namely, the tarantula and avicularia of Cayenne. Swammerdam, Roſſi, and Baglivi have left us little to wiſh for in this matter, as the effects of their bite and the remedies are both known.

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* Abridged from the Journal de Phyſique, LVIII. 463.

Spiders' webs a popular remedy for ſlight wounds.

The webs of ſpiders are conſidered by the common people as a remedy for wounds; country people often apply them on cuts or ſlight wounds, and apparently with ſucceſs. This property was not of ſufficient importance to induce chemiſts to analyſe the material; but as there has alſo been attributed to them a febrifuge virtue, ſuperior in ſome circumſtances to the bark, I have thought them entitled to a more particular examination. The following extract is taken from the *Journal d'Economie Rurale*, for Germinal, in the year XII.

Narrative of fevers cured by ſpiders internally taken.

" We have ſeen upwards of thirty years ago, a good prior, the curate of Batheren in Franche Comté, cure all the fevers of his pariſh, and of the neighbouring villages, by pills of a ſtrange compoſition. He went into his barn and formed ſmall pills with ſpiders' webs, by rolling them between his hands in the ſtate he found them. He adminiſtered this remedy to his patients in white wine, and very ſeldom failed to cure. M. Marie de St. Urſin being chief phyſician of the Hotel de Dieu, of Chartres, treated a very obſtinate fever in that hoſpital. He had employed bitters, the bark, and all the remedies of medical art without ſucceſs, when one of the female attendants offered to undertake the caſe with a certainty of cure. When ſhe was interrogated concerning her remedy, ſhe refuſed to mention it. M. de St. Urſin therefore continued to attend his patient for ſome days; after which, having a good opinion of the attendant, he determined to put his patient under her care. There was no return of the fever after the firſt doſe of the remedy. The phyſician ſuppoſed that the imagination of the patient, his confidence in a new remedy, and particularly the ſecrecy, might have ſuſpended the attack, and he waited, but to no purpoſe, for its return. The attendant encouraged by her ſucceſs, conſented to mention the remedy, which proved to be the ſame as that of the curate of Batheren."

Suppoſed to be effected by gelatine.

The editor of the Journal here quoted, being ſtruck with the new experiments of Seguin upon gelatine applied to the treatment of intermitting fevers, ſuſpects that ſpiders' webs may contain a principle reſembling animal jelly. The experiments of Cadet, while they overthrow this ſuppoſition, appear to him entitled to the attention of medical men.

Analyſis of ſpiders' webs.

Experiment 1. Spiders' webs triturated in the cold with quick-lime, emit a ſlight ammoniacal ſmell. 2. Cold water by digeſtion on the webs, becomes of a red-brown colour; is ſlightly precipitated by infuſion of nut-galls; is precipitated by acids; and this precipitate is again diſſolved when the acids are ſaturated with ammonia. 3. Spiders' webs cleaned as much as poſſible from duſt and foreign matters, were boiled in diſtilled water. The decoction ſmelled like champignons, and lathered by agitation. The undiſſolved matter was boiled in additional waters, until it gave out nothing more. All theſe waters being put together and evaporated, let fall their contents in ſucceſſive pellicles; and at length, by gentle evaporation, a ſolid extract was had, nearly equal to half the weight of the ſpiders' webs. 4. The reſidue not diſſolved in boiling water, was digeſted in alcohol. It gave a very deep orange-coloured tincture, which did not lather. Water being added, threw down a grey flaky precipitate, of a brown colour when dry, and little more than one hundred and ſeventieth part of the original webs. On hot coals it ſwelled up, ſmoaked, and took fire; and from its habitudes in theſe reſpects, and with the alkalies, it reſembled a reſin. The diluted alcoholic ſolution being then evaporated, afforded a reſidue ſlightly deliqueſcent, of a taſte at firſt ſweetiſh, and afterwards bitter, and in quantity nearly three times that of the reſinous precipitate. 5. The inſoluble reſidue after this treatment with water and alcohol, burned without ſwelling up, and emitted a ſmall quantity of white fumes having the ſmell of burned wood. Neither the oxigenated muriatic, nor the ſulphureous acids, diſcoloured it. It was ſoluble with efferveſcence in muriatic acid, which took up two-thirds and left

Analyſis of ſpiders' webs.

left a black paſte. Ammonia ſeparated a brown matter in ſmall quantity from the clear ſolution; and this matter, when calcined, did not loſe its colour. It was almoſt totally ſoluble in muriatic acid, and this ſolution gave a black precipitate with nut-galls, and a blue with alkaline pruſſiate. The fluid to which the ammonia had been added, gave a grey precipitate by pot-aſh. This retained its colour when ignited, and was again ſoluble in muriatic acid with efferveſcence. 6. Cauſtic pot-aſh poured on the reſidue of ſpiders' webs previouſly treated with water and alcohol, diſengages a little ammonia, and partly diſſolves the matter. An acid throws down from this ſolution a black pulvurelent taſteleſs precipitate, which ſlightly puffs up by heat, and leaves by deſiccation a brittle and apparently reſinous matter. Its quantity is about one-twelfth of the exhauſted matter made uſe of. It is partly ſoluble in volatile oils.

7. The aqueous extract of No. 3 being digeſted with alcohol, gave out one-ſeventh part. This alcoholic extract was brown, conſiderably deliqueſcent, and of a ſharp taſte. It ſwelled conſiderably on the coals, and at a certain period it burned rapidly, as if a nitrate were preſent. It efferveſced briſkly with ſulphuric acid, giving out a white vapour of a muriatic ſmell. Potaſh and lime diſengaged from this extract a ſtrong ammoniacal ſmell, and the vapours were very ſenſible on the approach of muriatic acid. The extract having been incinerated, appeared by ſeveral experiments to contain muriate of lime and a ſulphate. What remained of the aqueous extract after treatment with alcohol, was leſs deep in colour than before, had a purvurelent appearance, and ſlightly pungent taſte. On hot coals it did not ſwell up, but left a very abundant precipitate. Strong ſulphuric acid poured on this extract produced no ſenſible ſmell, and there was no production of ammonia when it was triturated with quick-lime.

8. Spiders' webs ſubjected to deſtructive diſtillation, gave firſt water ſlightly coloured, but becoming deeper as the proceſs went on; and afterwards a black thick oil, with carbonated hydrogen and carbonic acid. A very ſenſible ſmell of ammonia was developed, and a reſidual coal was left, amounting to half the matter employed. The coal after incineration left two-thirds of its weight, half of which was taken up by muriatic acid, and the remainder ſeemed to be ſilex and coally matter.

Analyſis of ſpiders' webs.

matter. The muriatic ſolution, during evaporation, depoſited ſulphate of lime. When ſpiders' webs were incinerated in an open veſſel, the aſhes were found to contain ſulphate of lime, muriate of ſoda, and carbonate of ſoda. Muriatic acid applied to the reſidue took up more ſulphate of lime; and when this ſolution was treated with ammonia and afterwards with pot-aſh, it gave oxide of iron, a little alumine, and ſome lime. The undiſſolved part was ſilex.

9. Spiders' webs were almoſt totally diſſolved in nitric acid amounting to ſix times their weight; carbonic acid and nitrous gas being diſengaged. The ſolution when evaporated let fall cryſtals of ſulphate of lime, and by continuing the evaporation, the yellow, bitter, deliqueſcent matter, which Welter calls *amer*, was afforded.

Component parts.

Hence the author concludes that ſpiders' webs are compoſed of, 1. A brown extract ſoluble in water, and not changeable in the air; 2. A reſinous extract ſoluble in alcohol, and very deliqueſcent; 3. A ſmall quantity of alumine; 4. Sulphate of lime; 5. Carbonate of ſoda; 6. Muriate of ſoda; 7. Carbonate of lime; 8. Iron; 9. Silex. The author thinks that the earths and earthy ſalts may be derived from the local ſituation of theſe inſects, and that it is probable that the webs of garden ſpiders may not afford them. The two conſtant products to which he demands particular attention, are thoſe obtained from the aqueous and alcoholic ſolutions. He thinks it deſirable to try their medical powers ſeparately. He ſuppoſes the reſinous matter to be the ſame ſubſtance as under other circumſtances forms the ſpiders' ſilk, and the wax which Mr. Accum has elſewhere mentioned as one of their products.

XII.

Information on the Mines and Manufactures of the Eaſt Indies, and other Subjects. By J. MACHLACHLAN, *Eſq. of Calcutta.**

SIR,

Account of ſome receipts for dying.

SHOULD you think the encloſed receipts for dying the beautiful reds of the Coromandel coaſt can be of any uſe to the dyers of the united Britiſh kingdom, be pleaſed to lay them before the Society for the Encouragement of Arts, &c. that they may be publiſhed in the volume of their Tranſactions; if not, I truſt you will excuſe my troubling you with them. They were ſent to me from Madras by a ſcientific friend, who had the ſeveral operations, detailed in them, performed in his own preſence. I forwarded a copy of them, and a ſmall quantity of the ingredients mentioned in them, to a friend at home, ſeveral years ago; but he dying about or ſoon after the time of their arrival, I never learned what became of them. It ſtrikes me, however, that there is a conſiderable coincidence between the thread proceſs and that which I have ſeen recommended by Mr. Henry, of Mancheſter, for dying the Adrianople or Turkey red.

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* Soc. Arts, 1804; for which the ſilver medal was given.

Immenſe quantities of talc found in the hills in Bahar.

I am not certain whether it is known at home, that many of the hills in Bahar, and other parts of India, contain immenſe quantities of mica, talc, or Muſcovy glaſs. The natives of this country and China make very ſplendid lanterns, ſhades, and ornaments of it, tinged of various fanciful colours; and it is alſo uſed by them in medicine. When burned or calcined, it is, I am told, conſidered as a ſpecific in obſtinate coughs and conſumptions. When powdered, it ſerves to ſilver the Indian paper, &c. uſed in letter-writing; and, in fact, it is applied to numberleſs purpoſes. The bazar price of that of the beſt quality, ſplit into ſheets of about two lines thick, is ſix rupees the maund of 84 lb. avoirdupois. If it could be applied to any uſeful purpoſe at home, it might go in part ballaſt of ſhips, and at a trifling expenſe. I encloſe a ſmall ſpecimen of it, and am,

Sir,

Your very obedient ſervant,

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

N. B. The chaya, or red dye root of the Coaſt, is, I believe, known at home: as alſo the caſhaw leaves, which are uſed as an aſtringent.

CHARLES TAYLOR, Eſq.

Directions

Directions for dying a bright Red, four Yards of ¾ broad Cotton Cloth.

Inſtructions for dying cottons bright red by the Indian method.

1ſt. The cloth is to be well waſhed and dried, for the purpoſe of clearing it of lime and congee, or ſtarch, generally uſed in India for bleaching and dreſſing cloths; then put into an earthen veſſel, containing twelve ounces of chaya or red dye root, with a gallon of water, and allow it to boil a ſhort time over the fire.

2d. The cloth being taken out, waſhed in clean water, and dried in the ſun, is again put into a pot with one ounce of myrabolans, or galls coarſely powdered, and a gallon of clear water, and allowed to boil to one half: when cool, add to the mixture a quarter of a pint of buffalo's milk. The cloth being fully ſoaked in this, take it out, and dry it in the ſun.

3d. Waſh the cloth again in clear cold water, and dry it in the ſun; then immerſe it into a gallon of water, a quarter of a pint of buffalo's milk, and a quarter of an ounce of the powdered galls. Soak well in this mixture, and dry in the ſun. The cloth, at this ſtage of the proceſs, feeling rough and hard, is to be rolled up and beetled till it becomes ſoft.

4th. Infuſe into ſix quarts of cold water, ſix ounces of red wood ſhavings, and allow it to remain ſo two days. On the third day boil it down to two-thirds the quantity, when the liquor will appear of a good bright red colour. To every quart of this, before it cools, add a quarter of an ounce of powdered alum; ſoak in it your cloth twice over, drying it between each time in the ſhade.

5th. After three days waſh in clean water, and half dry in the ſun; then immerſe the cloth into five gallons of water, at about the temperature of 120 degrees of Fahrenheit, adding 50 ounces of powdered chaya, and allowing the whole to boil for three hours; take the pot off the fire, but let the cloth remain in it until the liquor is perfectly cool; then wring it gently, and hang it up in the ſun to dry.

6th. Mix intimately together, by hand, about a pint meaſure of freſh ſheep's dung, with a gallon of cold water, in which ſoak the cloth thoroughly, and immediately take it out, and dry it in the ſun.

Inſtructions for dying cottons bright red by the Indian method.

7th. Waſh the cloth well in clean water, and ſpread it out in the ſun on a ſand-bank (which in India is univerſally preferred to a graſs-plat) for ſix hours, ſprinkling it from time to time, as it dries, with clean water; for the purpoſe of finiſhing and perfecting the colour, which will be of a very fine bright red.

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

CHARLES TAYLOR, Eſq.

Directions for dying of a beautiful red, eight ounces of Cotton Thread.

1ſt. Put one gallon and a half, by meaſure, of ſap-wood aſhes, into an earthen pot, with three gallons of water, and allow the mixture to remain twenty-four hours to perfect it for uſe.

2. Put the following articles into an earthen pot, viz. Three-quarters of a pint of Gingelly oil; one pint, by meaſure, of ſheep's dung, intimately mixed by hand in water; two pints of the above ley.—After mixing theſe ingredients well, pour the mixture gradually upon the thread into another veſſel, wetting it only as the thread, by being ſqueezed and rolled about by the hand, imbibes it, continuing to do ſo until the whole is completely ſoaked up, and allow the thread to remain in this ſtate until next day.

3d. Take it up, and put it in the ſun to dry; then take a pint and a half of aſh-ley, in which ſqueeze and roll the thread well and allow it to remain till next day.

4th. Squeeze and roll it in a like quantity of aſh-ley, and put it in the ſun to dry; when dry, ſqueeze and roll it again in the ley, and allow it to remain till next day.

5th. Let the ſame proceſs be repeated three or four times, and intermit till next day.

6th. Ley the thread once, as the day before, and, when well dried in the ſun, prepare the following liquor: One gill of Gingelly oil; one pint and a half of aſh-ley.—In this ſqueeze and roll the thread well, and leave it ſo till next day.

7th. Repeat the proceſs of yeſterday, and dry the thread in the ſun.

Inſtructions for dying cottons bright red by the Indian method.

8th. The ſame proceſs to be repeated.

9th. Firſt repeat the aſh-ley proceſs three or four times, as under the operations 3, 4, and 5, and then prepare the following mixture: On pint of ſheep-dung water; one gill of Gingelly oil; one pint and a half of aſh-ley.—In this ſqueeze and roll the thread well, and dry it in the ſun.

10th. Repeat the ſame proceſs.

11th. Do. Do.

12th. Do. Do.

13th. Do. Do.

14th. Do. Do.

15th. Waſh the thread in clean water, and ſqueeze and roll it in a cloth until almoſt dry; then put it into a veſſel containing a gill of powdered chaya root, one pint by meaſure of caſhan leaves, and ten pints of clear water; in this liquor ſqueeze and roll it about well, and allow it to remain ſo till next day.

16th. Wring the thread, and dry it in the ſun, and repeat again the whole of the 15th proceſs, leaving the thread to ſteep.

17th. Wring it well, dry it in the ſun, and repeat the ſame proceſs as the day before.

18th. Do. Do.

19th. Do. Do.

20. Wring and dry it in the ſun, and with the like quantity of chaya root in ten pints of water, boil the thread for three hours, and allow it to remain in the infuſion until cold.

21ſt. Waſh the thread well in clear water, dry it in the ſun, and the whole proceſs is complete.

J. MACLACHLAN.

Calcutta, Oct. 4, 1803.

SCIENTIFIC NEWS.

Imperial Academy of Sciences at Peterſburgh.

Prize of the Ruſſian Imperial Academy.

THE Vice Admiral Tchitchagoff, Miniſter of the Marine department, has forwarded to the Academy a queſtion on the reſiſtance of fluids, and its application to naval architecture, for the ſolution of which that department will beſtow a reward of 1000 ducats of Holland, or 462*l.* 10*s.*

The academy being deſirous of ſeconding the patriotic views of the marine department, decided on the publication of a program in the following terms.

Prize propoſed by the department of the Marine, on the 1ſt of *July*, 1804.

The ſubject. To improve the theories of the reſiſtance of fluids;

or to eſtabliſh a new theory;

or to deduce a formula from experiment.

It is propoſed, that of the two theories of the reſiſtance of fluids propoſed and applied to naval architecture by Don G. Juan, in his *Examen Maritime*, and by M. Romme, in his *Art de la Marine*, one or the other of them, for example, that of Don Juan ſhould be corrected and improved to ſuch a degree, as to afford reſults that ſhall differ from the reſults of experiment, by ſo ſmall a quantity, as may be practically neglected without ſenſible error:—Or otherwiſe, if theſe theories cannot be corrected, it is propoſed, that a new theory ſhould be eſtabliſhed and applied to naval architecture, which ſhall lead to concluſions of the ſame degree of accuracy;—Or otherwiſe, laſtly, if it ſhould be impoſſible to eſtabliſh ſuch a theory, it is propoſed, that from experiments at leaſt, there ſhould be deduced a formula reſembling thoſe which have been given by Meſſrs. Boſſut and Prony; and ſuch that it ſhall be not only more conformable to experiments than thoſe formulas, but that it ſhall lead as nearly as poſſible to the concluſions drawn from experiments, even when the formulæ ſhall be applied to naval architecture.

One thouſand ducats, or 462l. 10s.

For the ſatiſfactory ſolution of this problem the department of the marine has appointed a prize of 1000 Ducats of Holland, and has fixed a term of two years to be reckoned from the date of this program. After the expiration of that term, no memoirs addreſſed to the academy will be received on this ſubject, the time appointed being ſufficient for thoſe new experiments which the ſolutions in queſtion render indiſpenſably neceſſary. The memoirs forwarded to the academy muſt be written in a diſtinct legible character, either in the French Engliſh or Ruſſian language.

(Signed in the original) PAUL TCHITCHAGOFF.

Terms and conditions for receiving memoirs.

In the invitation accompanying this program, the Academy requires men of ſcience who intend to make application for this prize, to addreſs their memoirs to the perpetual ſecretary of that body, before the 1ſt of July, 1806, and that the writer ſhould clear the poſt charges as far as the regulations of their reſpective countries will allow. The cuſtomary mode of marking the memoirs with a device or motto, and ſending at the ſame time a ſealed letter, having the ſame device, and containing the name and reſidence of the author, is alſo to be adopted in the preſent inſtance. The memoirs will be examined before the expiration of the term of concurrence, by the Department of the Marine and by the Academy; the latter of whom will publiſh the judgment they ſhall adopt, and the Department of the Marine will crown by the payment of the prize, the labours of that author who ſhall have ſatisfied the conditions of the program.

Prize concerning Light, propoſed by the Imperial Academy of Sciences for the Year 1806.

Prize concerning light.

The uſual theories that light is projected matter; or mere undulation of a fluid;—

—or it is a chemical principle.

Five hundred roubles, £.112 10 0

After an introduction, in which a conciſe ſtatement is given of two theories reſpecting the nature of light, the one aſcribed to Newton, which ſuppoſes light to conſiſt in the emanations of matter from luminous objects, and the other aſcribed to Euler, which deduces the effects from vibrations of a peculiar elaſtic fluid,—they proceed to ſtate rather more fully a chemical hypotheſis of Lavoiſier, who not only conſiders light as cauſed by a peculiar matter, but alſo that this matter is ſubject to the elective attractions, ſo as by its combinations and diſengagements to produce an extenſive ſeries of phenomena, which are thus accounted for. It is principally with a view to develope this laſt hypotheſis that they have propoſed a prize of 500 roubles (£112 10 0.)

The queſtion.

" For the moſt inſtructive ſeries of new experiments on light, conſidered as matter; on the properties which may with juſtice be aſcribed to it; on the affinities which it ſhall appear to have with other bodies, whether organic or inorganic, and on the modifications and phenomena which are manifeſted in thoſe ſubſtances by virtue of the combinations in which the matter of light may have entered along with them."

Remarks by the Academy.

After propoſing this queſtion, the Academy proceeds to explain, by obſerving that without entering into any hiſtoric diſcuſſion, or the objections which have been oppoſed to this hypotheſis, nor the reſearches already made with a view to develope traces of chemical action between light and bodies in the different modifications of natural phenomena,—the enquiries here propoſed may not be unuſefully extended to the galvanic fire, of which the dazzling brilliancy when large piles are made to act upon coaly matters, in ſome reſpect imitates the ſolar light. The Academy has choſen to enunciate the ſubject of their prize in a general way, in order that philoſophers may not be in any reſpect impeded as to the points of view from which they may be diſpoſed to contemplate and to treat ſo difficult a ſubject, which has ſcarcely yet been entered upon, though ſo eminently worthy of attention from the cultivators of natural ſcience.

Conditions and terms.

The memoirs are to be written either in Ruſſian, French, Engliſh, German, or Latin, and forwarded to the perpetual ſecretary of the Academy, ſealed up, with device and indicatory billet, as mentioned with regard to the former prize. No memoirs will be received after the 30th of April, 1806, incluſive, and the author of that memoir which in the judgment of the Academy ſhall have merited the prize, ſhall be proclaimed in the public meeting of the following month of July. The ſucceſsful memoir becomes the property of the Academy, and muſt not be printed without their formal permiſſion. The other treatiſes will be delivered to the reſpective authors, on application to the ſecretary, either perſonally or by procuration.

Voyages of Meſſrs. HUMBOLDT *and* BONPLAND.

Voyage of Humboldt and Bonpland.

Meſſrs. Levrault, Schoell and Company circulated at the beginning of the preſent year a proſpectus of the voyage of Meſſrs. Humboldt and Bonpland, the publication of which is committed to them: "the travellers, they obſerve, have in general re-written all their obſervations, whatever might have been the object, in the narrative of their voyage. Mr. Humboldt has thought it proper to follow another courſe, and to treat

Humboldt's travels.

treat ſeparately all thoſe objects which conſiderably differ in their nature. He has determined accordingly to publiſh in detached collections, all that more particularly belongs to Aſtronomy, Geology, Botany, Zoology, &c. and his voyage, properly ſo called, will embrace all that relates to General Phyſics, the Origin of Nations, their Manners, their Intellectual Culture, Antiquities, Commerce, and Political Economy. Upon this part of his obſervations and the hiſtory of his voyage, he will not at preſent publiſh more than a narrative, under the title of *Relation abrégé*, &c. or an abridged Relation of a Voyage to the Tropics, performed in the interior of the new continent during the years 1799, 1800, 1801, 1802, and 1803.

It is agreed by Meſſrs. Humboldt and Bonpland, who are connected by the moſt intimate ſentiments of friendſhip, and have ſhared together in all the fatigues and dangers of this voyage, that the whole of their publications ſhall be in their joint names. The preface to each work will announce to which of them the ſeveral parts are reſpectively to be aſcribed. The liſt of works ſpeedily to appear are as follows:

1. The abridged Relation of the Voyage, in quarto; promiſed in July, 1805.

2. A Collection of Aſtronomical Obſervations and Admeaſurements made on the new continent; promiſed in 1805.

3. An Eſſay on the Geography of Plants, or a Philoſophical Sketch of the Equinoctial Regions; founded on obſervations made from the 15th degree of ſouth latitude, in the years before mentioned, with one large plate, coloured; promiſed in June, 1805.

4. Equinoctial Plants, collected in Mexico and the Iſle of Cuba, in the Provinces of Caracas, Cumena and Barcelona, on the Andes of New Grenada, Quito and Peru, and on the banks of Rio Negro, Oroonoko and the River of Amazons, with plates engraved by *Sellier*, in folio; the firſt number to appear in April, 1805.

5. A Collection of Obſervations of Zoology and comparative Anatomy, made in a Voyage to the Tropics; with plates engraved by *Bouquet*, coloured or not, at the option of the purchaſer. The firſt number to appear in May.

N. B. All theſe works will bear the general title of *Voyage de M. M. Alexandre de Humboldt et Aimé Bonpland*, and will form a collection of the ſame ſize and type, except the Equinoctial Plants, which are larger. Subſequent notices of the price and publication are to appear in the journals.

Fiſh ejected from Volcanoes.

Volcanic eruption of fiſhes.

AMONG the great number of facts which Humboldt has collected in his voyage, the following lately communicated to the National Inſtitute is very curious. Several volcanoes of the Cordilleras of the Andes occaſionally throw out eruptions of mud mixed with large volumes of freſh water, and what is moſt remarkable, an infinite number of fiſhes. The volcano of Imbaburo, among others, threw out at one time ſo great a number near the town of Ibarra, that their putrefaction occaſioned diſorders. This phenomenon, aſtoniſhing as it appears, is not even extraordinary, but, on the contrary, of conſiderable frequency, ſo that the facts are authentically preſerved in the public regiſters, along with thoſe of earthquakes. It is more particularly ſingular that theſe fiſh are not at all injured, though their ſtructure is very ſoft. They do not even appear to have been expoſed to a high temperature; for the Indians aſſert that they ſometimes arrive at the foot of the mountain ſtill living. Theſe animals are ſometimes thrown out of the mouths of the crater and ſometimes through lateral clefts; but always at the height of 12 or 1300 toiſes or fathoms above the ſurrounding plains. Humboldt thinks they are produced in lakes ſituated at that height within the crater; and it is a confirmation of this opinion that the ſame ſpecies are found in the brooks which run at the foot of the mountains. It is the only ſpecies which ſubſiſts at the height of 1400 toiſes. It is a new ſpecies to naturaliſts. Humboldt made a drawing of it on the ſpot, and gave it the name of *Pimelodrus Cyclopum* or *thrown by the Cyclops*. It will be found in the firſt number of his Zoology.

Water formed by Mechanical Preſſure.

Combuſtion of oxygen and hydrogen by mechanical preſſure.

IN a ſitting of the French National Inſtitute at the commencement of the preſent year, M. Biot read a note on the formation of water by mere compreſſion. The experiment of forming water out of its component parts oxygen and hydrogen by burning thoſe gaſes by the electric ſpark is well known. M. Biot has ſucceeded in determining this combination independently of electricity, by rapidly compreſſing a mixture of the two gaſes included in the ſyringe of an air-gun. The compreſſion which forces the particles of gas together, cauſes them to give out a ſufficient quantity of heat to ſet them on fire. Some caution is requiſite in repeating this experiment, which is not without danger. Out of three times that M. Biot made it, there were two in which the braſs cap of the pump and the barrel itſelf, which was iron, were broken by the exploſion.

Malleability of Zinc.

Zinc is malleable while heated between 210° and 300°.

A VERY curious and uſeful diſcovery has been made by Meſſrs. Charles Hobſon and Charles Sylveſter, both of Sheffield, that zinc is in fact a malleable metal. The laminability of this metal to a certain conſiderable degree, has long been known; but it was not ſuſpected that it is capable of being forged and drawn into wire. They have found that at a temperature between 210° and 300° of Fahrenheit zinc yields to the hammer, and alſo that it may be wire-drawn or laminated by keeping it at this temperature during the mechanical operation. An oven or a hollow metallic veſſel kept at a due heat may be uſed for the pieces, in the ſame manner as the Smith's forge is uſed for iron and ſteel. It appears that the zinc, after having been thus annealed and wrought, continues ſoft, flexible and extenſible, and does not return to its former partial brittleneſs, but may be bended and applied to the uſes for which zinc has hitherto been thought unfit, ſuch as the fabrication of veſſels, the ſheathing of ſhips, and numerous other important applications. I have ſeen a chaſed or ſtamped figure raiſed at one ſtroke in thin zinc; which is, I think, as much elevated as it could have been in copper.*

Palladium.

Palladium.

BY a letter from Meſſrs. Knight, of Foſter-lane, I am informed that the new metal, palladium, may be purchaſed at their warehouſe.

Erratum corected.

MR. GREGORY begs leave to correct a miſtake which occurs in his paper on horſe-powers, occaſioned by his inadvertently copying from ſome of his former obſervations on that ſubject the fraction $\frac{9}{4}$ inſtead of $\frac{9}{5}$ for the approximate value of the exponent n: this makes a change in the reſults of the computation at page 148; for $(9-3)^{\frac{9}{5}} : (9-4.4)^{\frac{9}{5}} :: 130 : 80{\cdot}58$, or nearly $80\frac{1}{2}$ lb. inſtead of $71\frac{1}{2}$, as there given. He alſo points out a preſs error at p. 153, where the ſymbols of multiplication in each of the theorems ſhould have been ſigns of addition.

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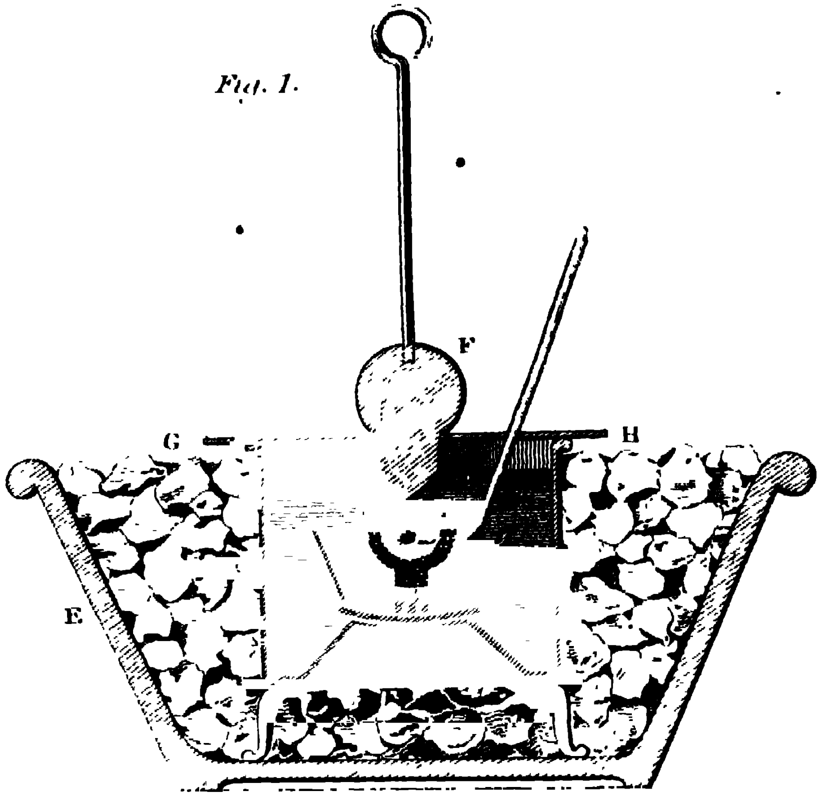
* The inventors have obtained a patent.

New Experiments by Count Rumford to prove the maximum of Density in Water above 32° fahr.

Fig. 1.



Chevalier Edelcrantz's Valves for Steam Engines

Fig. 2.

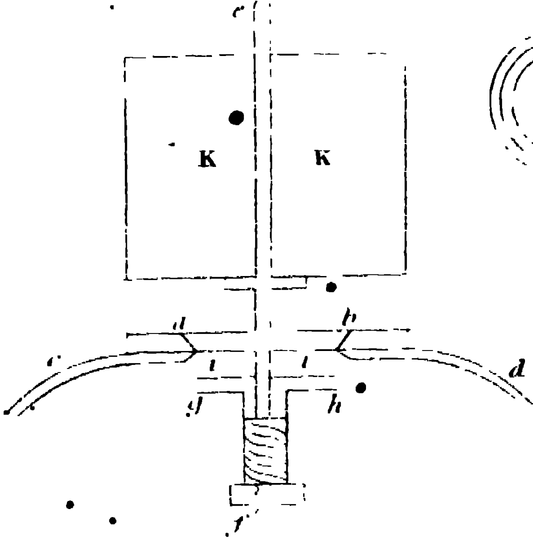


Fig. 3

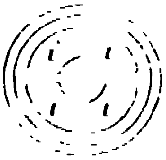
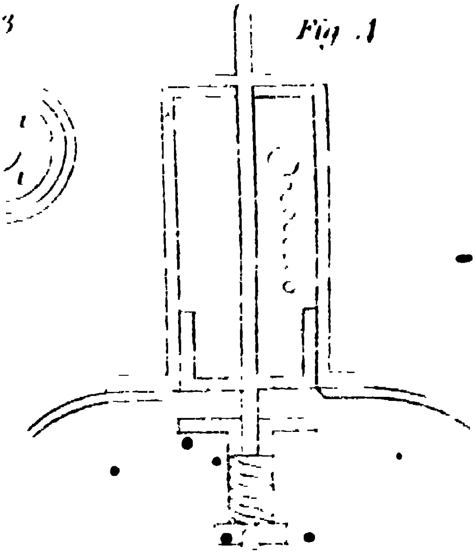


Fig. 4



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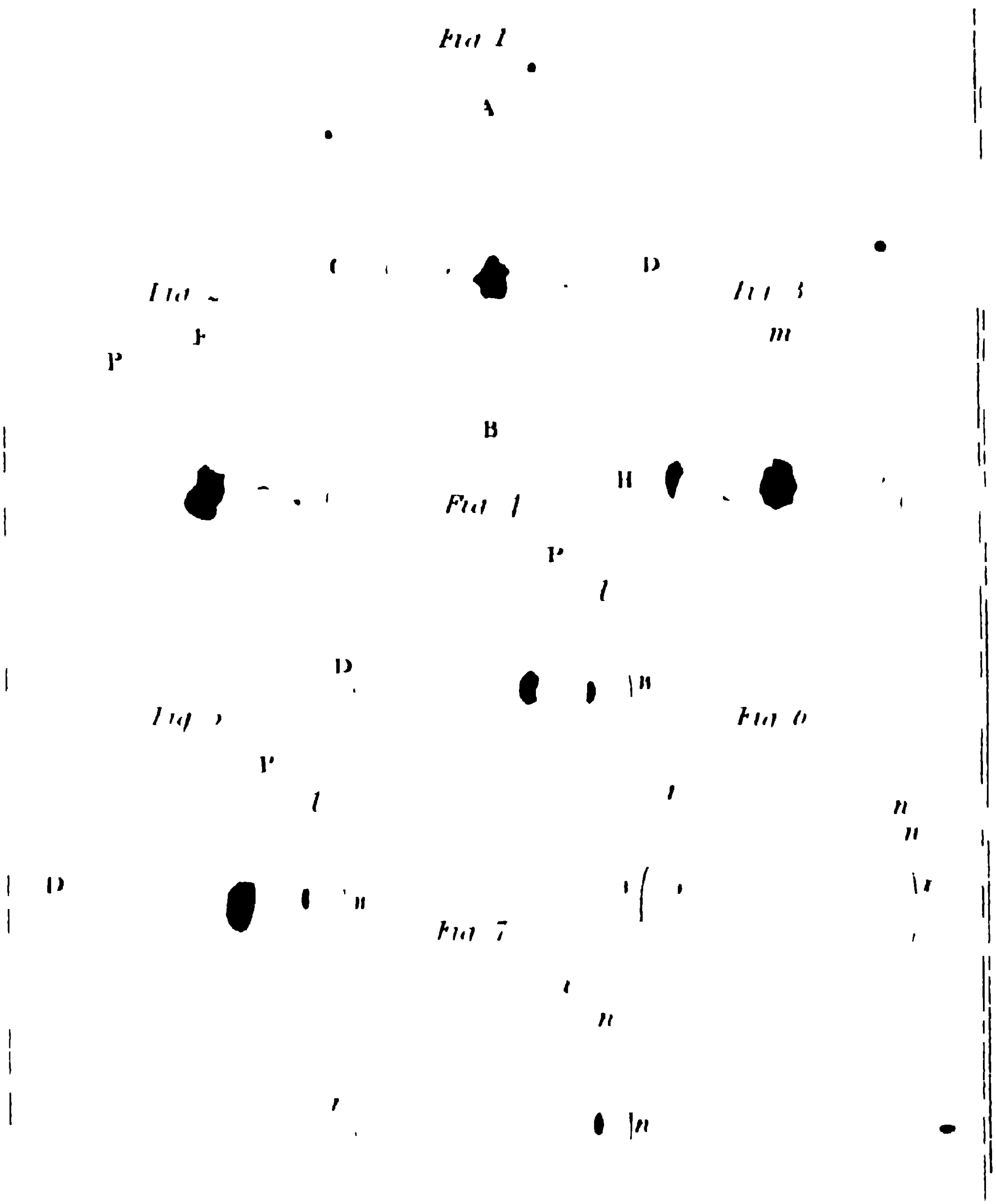
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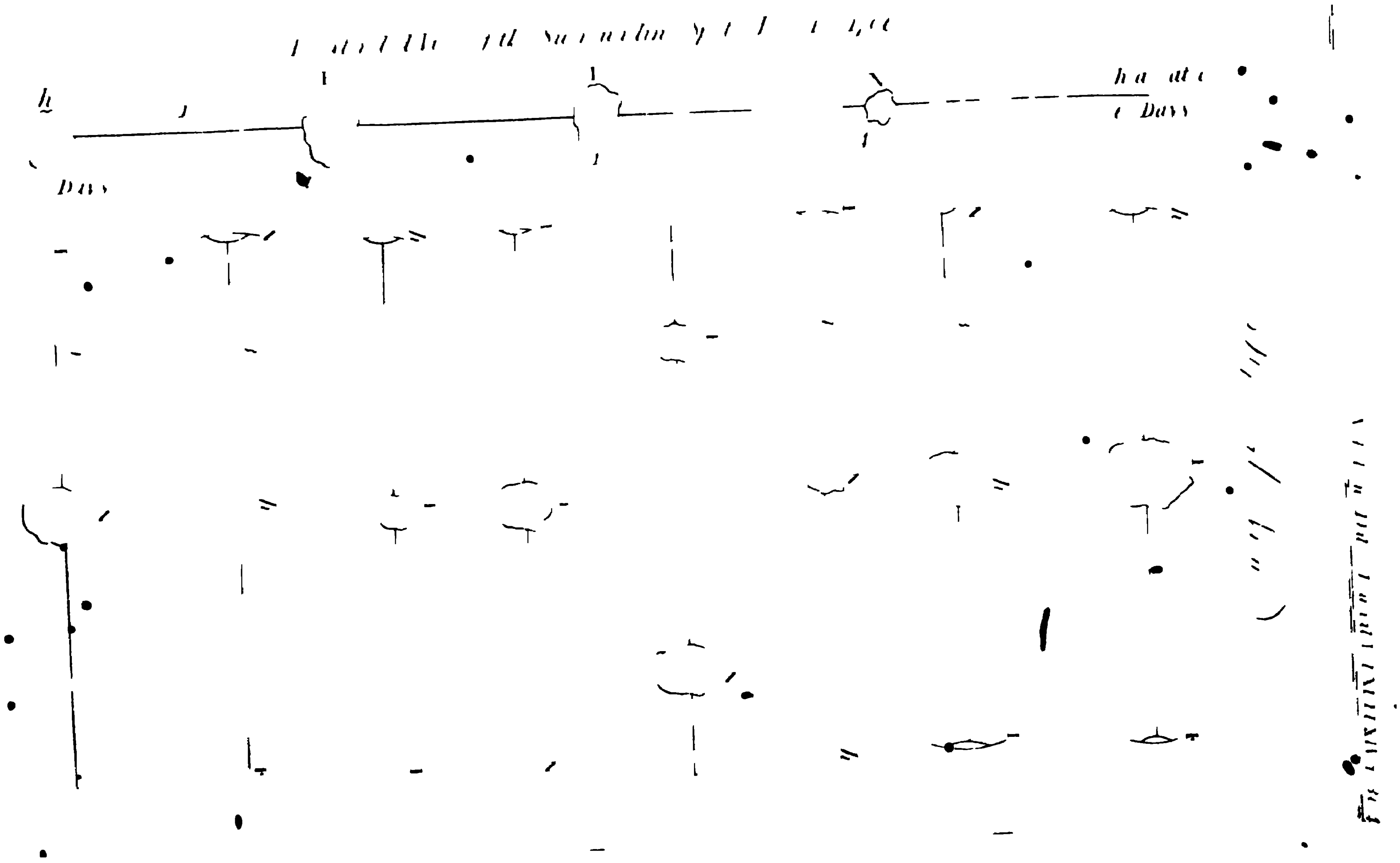
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Days

C. Mr. Harrison's Press for Botanical Specimens

Fig 1

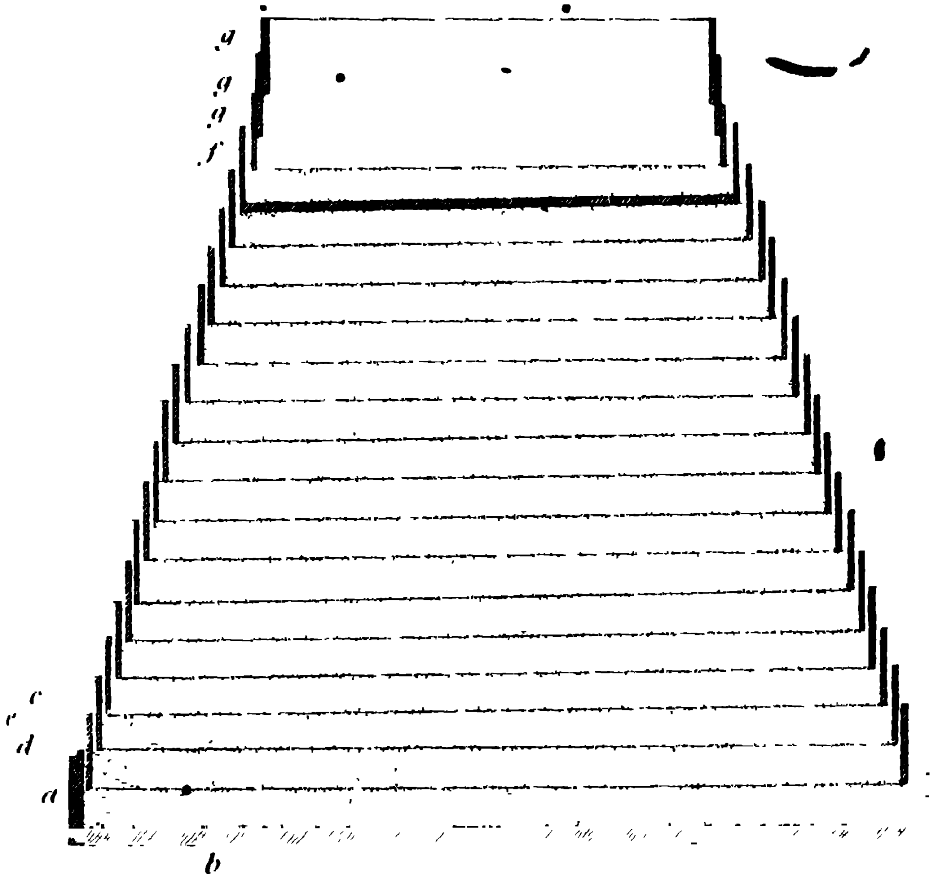


Fig 2.

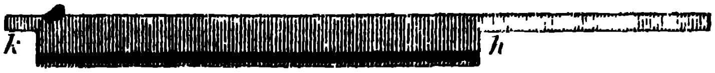
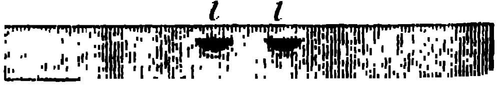


Fig 3



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